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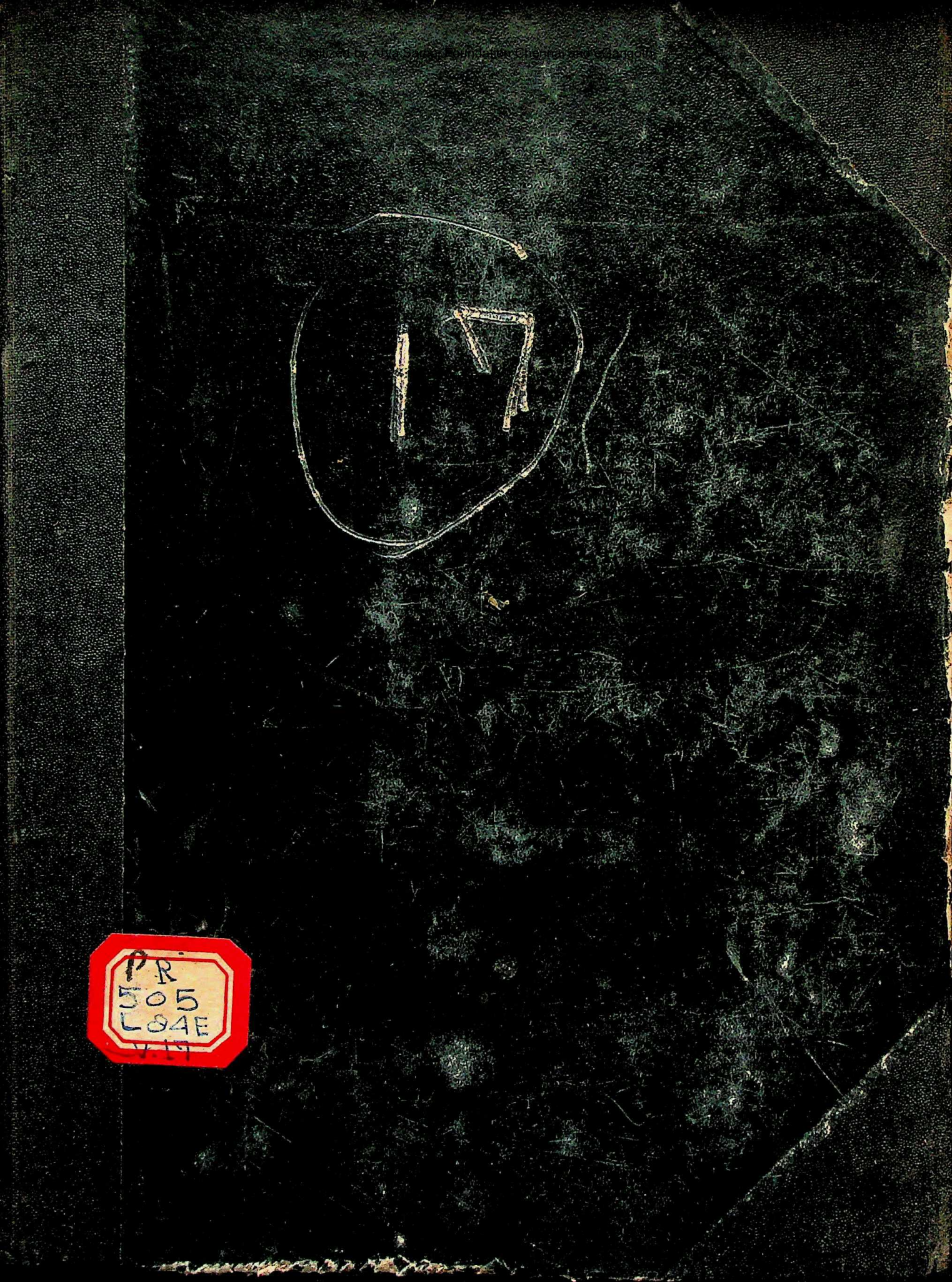

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ENDEAVOUR

A quarterly review designed to record the progress of the sciences in the service of mankind

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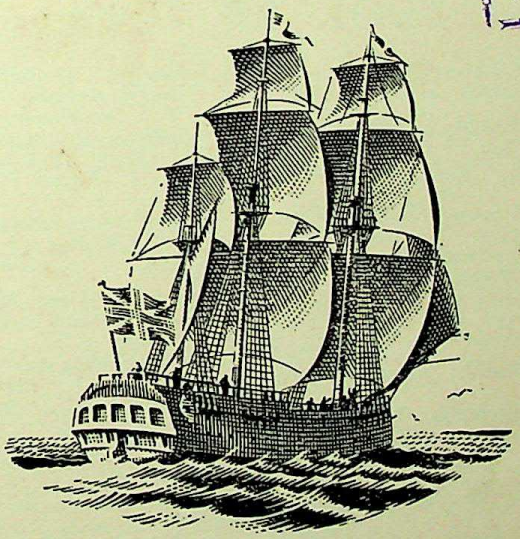

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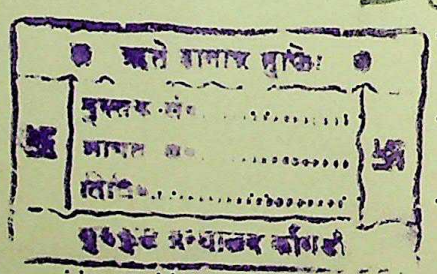

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Some Irish scientists

पुस्तकालय

The recent meeting in Dublin of the British Association for the Advancement of Science gave many reminders of important contributions by Irish scientists. The quality of their work is noteworthy for so small a country and deserves wider appreciation: this is an appropriate occasion on which to recall some of them and their achievements.

Although the family was originally a Herefordshire one, such an account may well begin with mention of Robert Boyle (1626–1691), seventh son of the first Earl of Cork and Orrery, born at Lismore Castle, Munster. His work was largely done in England, partly because he found the Ireland of his day too troubled 'to have any Hermetic thoughts in it', but he greatly influenced the course of development of Irish science. His work inspired the formation in 1683 of the Dublin Philosophical Society, which, although itself short-lived, was the forerunner of the Royal Dublin Society (1731) and the Royal Irish Academy (1785). Boyle has been fairly described as 'the father of modern chemistry': it is worth noting that the Irish chemist William MacNeven (1763–1841), professor of both chemistry and materia medica in the College of Physicians and Surgeons in New York, has been described as 'the father of American chemistry'.

Boyle's genius is hard to match in any age, but nevertheless Ireland provided a number of able successors. Notable among them was the eccentric Richard Kirwan (1733–1812), 'the Philosopher of Dublin'. Kirwan spent the years 1777–1787 in London and was there in touch with many of the most eminent chemists of the day—men such as Joseph Black (himself Belfast-born), Henry Cavendish, and Joseph Priestley—at a time when chemical thought was in a ferment. Returning to Dublin fresh from these stimulating contacts, he did much to arouse chemical interest there, especially through the Royal Irish Academy, of which he was president for the last thirteen years of his life. In London, Kirwan was a convinced believer in the phlogiston theory—which though erroneous had much to commend it in its day—and his 'Essay on Phlogiston', published in 1787, was very widely acclaimed. Lavoisier and his associates, however, had the book translated into French and then had it published with a chapter-by-chapter refutation. Kirwan accepted the refutation 'with the candour which distinguishes superior minds'—unlike Priestley, who continued to advocate the phlogiston theory until the day of his death. Kirwan's other chemical work, of a calibre to earn him the Copley Medal of the Royal Society, was concerned with such diverse subjects as chemical affinity, mineralogy, dyeing and bleaching, and agricultural chemistry.

William Higgins (1763–1825) is a controversial figure, but nevertheless a great chemist. For the last thirty years of his life he was chemist to the Irish Linen Board and, through the good offices of Kirwan, professor of chemistry and mineralogy in the Dublin Society: the latter association was a stormy one. During the past 150 years many claims have been made on Higgins's behalf that in his 'Comparative View' of 1789—primarily another refutation of Kirwan's 'Essay on Phlogiston'—he anticipated Dalton as originator of the atomic theory. The validity of these claims is hard to assess, but they are certainly not without substance: one notable authority has summarized the argument by saying that it would be fair, if not now practicable, to call the theory the Higgins-Dalton theory. It may be remarked that Higgins's ideas on the nature of the atom were undoubtedly inspired in some measure by his uncle Bryan Higgins (1741–1818), who had a chemical laboratory in Soho, London, in which William worked before going to Oxford.

Another able Irish chemist, and a notable eccentric, was Peter Woulfe (1727–1803). He too was a Copley Medallist of the Royal Society, but is best remembered by the Woulfe bottle, known to every student of chemistry, even though this was invented by Glauber. He was most probably one of the chemists before whom Dr Price of Guildford, a Fellow of the Royal Society, was in 1793 required to demonstrate his claim to be able to transmute base metal into gold. Price's nerve failed him, and he committed suicide before the demonstration.

None but the briefest list of distinguished chemists could omit Sir Robert Kane (1809–1890). By the age of thirty-five he had achieved an international reputation as a chemist—he was a Royal Medallist of the Royal Society—and then achieved equal distinction as an Irish educationist and administrator. His book on 'The Industrial Resources of Ireland' was acknowledged as masterly. Nor should James Muspratt (1793–1886) be omitted. Born in Dublin, he founded the alkali industry in England. His sons Edmund Knowles and James Sheridan were also notable chemists.

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Josias Gamble (1776–1848), another great pioneer of the British chemical industry, and for a time a partner of James Muspratt, was an Enniskillen man and before settling at St Helens in Lancashire had chemical factories in Monaghan and Dublin. James Mallet (1832–1912), a graduate of Trinity College, Dublin, held a number of important chairs of chemistry in the United States. In 1882 he was elected president of the American Chemical Society, a notable tribute to a non-American who had fought on the losing side in the Civil War only seventeen years previously.

In the field of mathematics and physics, too, there have been many Irishmen of distinction. Perhaps the greatest man of science that Ireland has produced is William Rowan Hamilton (1805–1865). His early life was remarkable. At the age of thirteen he knew as many languages, including Arabic, Sanskrit, and Persian. He was elected to the chair of astronomy at Trinity College, with which went the title of Astronomer Royal of Ireland, while still an undergraduate, and while in this status had sometimes to examine graduates. His early researches led to the important discovery of conical refraction, but his great discovery was that of the calculus of quaternions in 1843. The last twenty-two years of his life he devoted mainly to elaborating this, and applying it to problems in dynamics, astronomy, and wave theory. It may be mentioned in passing that his English-born contemporary George Boole (1815–1869) founded mathematical logic while professor of mathematics at Cork: he was also the discoverer of algebraical invariants.

John Tyndall (1820–1893), born at Leighlin Bridge, was a man of wide talent, and succeeded Faraday at the Royal Institution. He is remembered most for his work on radiant heat, the absorption of heat by gases, and the scattering of light. He also made a great name for himself as a popular expositor of science. Thomas Andrews (1813–1885) of Belfast made important studies of the liquefaction of gases and was the discoverer of the phenomenon of critical temperature. Among the most brilliant of mechanical engineers was Charles Parsons (1854–1931), inventor of the steam turbine. His scientific ability was doubtless at least partly inherited, for he was the sixth son of the third Earl of Rosse, an astronomer of distinction, whose great 72-inch reflecting telescope, made in the castle workshops at Parsonstown, was for many years unrivalled. With this instrument was discovered the spiral structure of certain of the white nebulae.

The 'FitzGerald contraction' commemorates the genius of C. F. FitzGerald (1851–1901), professor of natural and experimental philosophy at Trinity College. Profoundly interested in the problem of the aether, he sought an explanation of the well-known Michelson-Morley anomaly. In 1891 FitzGerald pointed out that this anomaly disappeared if it were assumed that all bodies contracted in the direction of their motion. He developed this idea quantitatively, and calculated the theoretical degree of contraction. This conception, developed by the mathematician Joseph Larmor (1857–1942) among others, was subsequently embodied in the general theory of relativity.

Among others deserving of mention in the present context is John Joly (1857–1933), who was both physicist and geologist. He was the originator of the theory of thermal cycles, based upon the widespread distribution of radioactive elements in the earth's crust; he made a famous attempt to calculate the age of the earth from the salinity of the sea; and he was a pioneer of colour photography. G. J. Stoney (1826–1911) of Galway has a lasting memorial in the word electron, which he introduced to designate the elementary charge of electricity. R. S. Ball (1840–1913) of Dublin had a distinguished career in Ireland as an astronomer before being appointed professor of astronomy and geometry at Cambridge. He contributed notably to the theory of screws and was a brilliant expositor of science. G. G. Stokes (1819–1903) had a distinguished career in mathematics and physics and made many important contributions in hydrodynamics and in the theory of light.

This catalogue of Irish scientists includes none whose work is not generally known. To them might be added many others, familiar to the historian of science, who have made lesser but by no means unimportant contributions—T. W. Callan, R. Chevenix, T. Rutty, C. Lucas, D. Macbride, R. L. Praeger, and W. Stokes, to mention but a few names at random.

The history of Ireland has long been a troubled one and, as Boyle remarked, often not conducive to original thought. Independence, long sought, has by no means solved all problems, but the meeting of the British Association, which suggested this survey, gave a welcome opportunity of seeing how the spirit of scientific inquiry lives on despite difficulties.

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Cosmology

W. H. McCREA

Astronomers are generally agreed upon some important inferences from observations of the universe. However, both optical and radio observations are still inconclusive in showing whether the universe is evolving or is in a steady state. Some observational and theoretical objections to steady-state cosmology have recently been refuted, and it appears to admit a more promising theory of galaxy formation than any proposed hitherto on the basis of evolutionary cosmology. An attempt is made here to give a brief general review of the subject.

The universe in the large is an inevitable subject for scientific curiosity, but the most commonplace physics also leads us to study it. For instance, a non-rotating frame of reference can ultimately be defined with regard to nothing less than the general background of the galaxies. This indicates at least one way in which the universe in the large influences every laboratory experiment. More generally, we conceive that the laws of physics and the actually existing physical universe must be interdependent, so that physics is bound to merge into cosmology.

We may conveniently define cosmology to be the study of the astronomical universe as the system of the galaxies and any material in intergalactic space. We are therefore not concerned with the internal affairs of any particular galaxy unless they affect the behaviour of the rest of the system or provide information about it. The present time is suitable for taking stock of the subject because there seems to be a halt in its development, although much that is happening in physics and astronomy can be seen to have a bearing upon it.

THE UNIVERSE OF GALAXIES

The lines in the optical spectra of galaxies show a shift towards the red that increases with decreasing apparent brightness. The facts are consistent, to a good first approximation, with the interpretation that the galaxies are receding with speeds proportional to their distances. This interpretation is generally accepted. It received notable support when American observers [1, 2] recently found, in the few cases so far examined, that the 21 cm line in the radio spectrum showed velocity shifts agreeing well with those in the optical spectrum. The constant of proportionality in the velocity law (Hubble's law) is written $1/\tau$; it means that if the galaxies have always had their presently observed velocities they would all have coincided in space τ years ago. The current estimate [3] of τ, based upon W. Baade's important revision of the distance-scale, is $5{\cdot}4 \times 10^9$ years. In our discussion we shall use 6×10^9 years as a round figure, since the tendency is towards larger estimates.

The fact that so general a property as the motion of the galaxies is found to be the same for all directions strongly indicates that the universe as a whole must be isotropic about our position. If the universe were not homogeneous but happened to be isotropic about ourselves, we should be in a highly favoured position. Since for philosophical reasons it is difficult to accept this, we must conclude that the universe is homogeneous, and therefore isotropic about any position. The number of galaxies accessible to present means of observation is of the order of 10^{10}. Because of practical difficulties, what is known observationally about their distribution does not yet suffice for making direct inferences. It does not, however conflict with the inference of large-scale homogeneity.

The homogeneity signifies that an observer in any other galaxy would get the same general picture of the universe as we do if the universe is in a steady state, or the same sequence of pictures if it is evolving. If valid, this is the most important thing we know about the universe. For it means that we are in a position to know all that can be known about the astronomical universe, in the sense that we could learn nothing more about its large-scale behaviour by going anywhere else within it. If the universe is in a steady state, we could moreover learn no more by living at another epoch, but this would not follow if the universe is evolving. It is in fact hard to see any justification for accepting the 'universality of physical law' unless there is also universality of physical background, i.e. homogeneity of the universe in the large. It is satisfactory to find evidence for the

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latter, at any rate as regards space; whether it applies also to time is one of the main points to be discussed.

It must be remarked that the relatively small-scale distribution of galaxies is irregular. Many belong to clusters having from two or three up to about one thousand members; others cannot be assigned to particular clusters. The Milky Way Galaxy, for instance, belongs to the Local Group of some twenty members. The galaxies of a cluster are gravitationally bound to the cluster; they cannot recede from each other in consequence of the general cosmical expansion, just as the Earth cannot recede from the Sun.

THEORIES

The most obvious explanation of the apparent recession of the galaxies is that we are witnessing a one-way evolution of a universe whose contents are given once and for all. The mechanical behaviour of such a system is appropriately treated by Einstein's theory of general relativity. For simplicity, local irregularities of the contents are ignored: we consider the smoothed-out universe. The equations obtained have a range of solutions. Those that come nearest to fitting the observations all require the material to have been in a state of enormous density, infinite in the idealized model, rather less than τ years ago. The production of this singular state is given no meaning in the theory; it must be regarded as the creation of the universe, and a unique 'age of the universe' must be dated from it.

On the other hand, the essentially simplest view is that we should suppose there to be nothing to distinguish our position in time from any other, just as we have already inferred that there is nothing to distinguish our position in space. Thus, we should suppose the universe to be in a steady state. This refers to the universe in the large or to the smoothed-out universe.

The steady-state theory of the expanding universe was propounded by H. Bondi and T. Gold and by F. Hoyle in 1948. For a general account the reader is referred to Bondi's book [4]. The writer has previously reviewed the theory in this journal [5]; the arguments previously given need not be repeated, but the main conclusions will be recalled in the course of the present discussion. Numerical values in the earlier accounts have to be revised to accord with Baade's new distance-scale.

According to the steady-state theory, existing galaxies are indeed dispersing. But new matter is continually appearing throughout the universe and giving rise to new galaxies, thereby keeping the population distribution steady. Any large region contains galaxies of all ages, but with an average that works out at $\frac{1}{3}\tau$ years. The general merits of this theory are that we avoid (*a*) the dubious procedure of employing unchanging laws in a changing universe; (*b*) the paradox of finding objects such as stars that are older than the universe; (*c*) the dilemma of having everything dependent upon a creation process that nevertheless is outside the scope of the theory.

If we observe a region of the universe, say, 10^9 light-years distant, this means that we see it as it was 10^9 years ago. According to evolutionary theory we should see galaxies 10^9 years younger than our own, and so on for other distances. We ought, therefore, to discern ever earlier stages of evolution with increasing distance. Such an effect is, however, clearly impossible in a steady-state universe. The detection of any age-distance effect alone would suffice to show that the universe is not in a steady state.

Moreover, steady-state theory gives an effectively unique model of the universe. It gives only one possible relation between red-shift and apparent magnitude of the galaxies, and so on, as compared with the range of relations in evolutionary theory. Therefore, were any one of these predictions found to disagree with observation, the steady-state hypothesis would have to be abandoned. This vulnerability of the theory has been emphasized particularly by Gold.

It is consequently convenient to survey the present position mainly from the standpoint of steady-state theory, simply because it offers the most crucial predictions for discussion.

TESTS OF THE THEORIES

Suppose we had in ordinary classical space and time a uniform distribution of galaxies all of the same age and all receding from us with speeds proportional to their distances. Using classical methods, we could calculate the curves for the relations between pairs of observable quantities, e.g. Doppler shift and apparent brightness. The cosmologies under review yield curves that in general differ from those so calculated and from each other. But the corresponding curves given by the different methods all have the same tangent at the origin. Moreover, the range of present observation takes in the parts of the curves that depart from the tangent by only small amounts. Relative to available observations, therefore, all

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theories make to the first order the same predictions, and discrimination between them depends upon effects of second or higher order near the limits of instrumental performance. But, because they are near these limits, the observations are subject to instrumental or selection effects that are probably of at least the same order.

Despite the superb work done by observers, it seems that, for these reasons, all efforts to discriminate between the theories by means of large-scale features must still be inconclusive. This seems to apply not only to optical observations but also to radio surveys. This is evident on comparing results by different observers [6, 7]. This limitation includes observations of properties that might reveal age-effects. Individual galaxies must evolve most rapidly in extreme youth, so that even if there is a correlation between age and distance, as required by evolutionary theory, its consequences could probably be detected only outside the range of existing instruments. The colours of certain galaxies were for a time believed to show what might be an age-effect, but the evidence has now been invalidated [8]. At present, therefore, we must consider less direct lines of evidence.

NUCLEAR SYNTHESIS

The relative abundances of the various species of atomic nuclei present in the Earth, meteorites, primary cosmic rays, solar and stellar atmospheres, and interstellar matter, show a definite correlation with nuclear properties. This indicates that the relative abundances must be the result of definite physical processes about which something ought to be discoverable. But the only known places in the universe where nuclear reactions can proceed are the stars, and until recently it was considered impossible for nuclear synthesis to proceed there beyond atomic number 8. The relative abundances were considered to be probably the same everywhere in the cosmos, apart from the effect of stellar processes upon the light nuclei. Everything, therefore, seemed to require a cosmic process of nuclear synthesis. If synthesis could not proceed in the cosmos as we know it, we had to conclude that the cosmos must have passed through a very different state in the past. The initial state of enormous density inferred from evolutionary theory seemed to provide the necessary conditions.

The theoretical possibilities of nuclear formation in the early stages of an 'exploding' universe have been thoroughly investigated, but the production of heavy nuclei could not be explained. G. Gamov, who did much of the work, now states: 'I would agree that the lion's share of the heavy elements may well have been formed later in the hot interiors of stars' [9]. This refers to a recent transformation of outlook on the astrophysical side. The brilliant investigations of E. E. Salpeter, F. Hoyle, W. A. Fowler, A. G. W. Cameron, and others have shown beyond question that nuclear synthesis can occur in stars after the exhaustion of hydrogen in the central regions [10]. There is a variety of astronomical evidence in support of the new ideas, which also tends to dispel the older view regarding 'cosmical' abundances of the elements.

The synthesis of atomic nuclei seems therefore to be a matter for individual galaxies and not to provide a criterion for cosmological theories.

FORMATION OF GALAXIES

According to evolutionary theory, galaxies must have been formed from a space-filling gas at a definite stage in the expansion of the universe. But a satisfactory process has not so far been proposed. Gamov's theory [11] demands a type of turbulence in the gas that appears to be as difficult to account for as the result it is designed to explain. D. Layzer [12] has given reasons for concluding that in an evolutionary universe the resolution of material into condensed bodies would take place in the order opposite to that usually assumed, i.e. that the gas would first produce dust, the dust would produce bodies like planets, these would produce stars, and the stars would form binaries, clusters, and finally galaxies and clusters of galaxies. While there are some broad considerations to support this idea, it has not yet shown itself susceptible of precise development.

By its nature, steady-state theory cannot pose the problem of an original formation of galaxies. D. W. Sciama has stressed that its problem is the self-perpetuation of the existing population with all its characteristics. These include the mass-function of the galaxies, the mean separation of the galaxies in space, the population-function of clusters of galaxies, the distribution of random motions of individual galaxies, and the mean density of intergalactic matter. Sciama has outlined a remarkable solution of the problem [13], which is briefly as follows.

A galaxy is, in general, in motion through intergalactic gas; this is by virtue of its random velocity and not of cosmic expansion, which applies equally to the gas. The gas that falls towards it as it passes, forms a wake behind the galaxy, as in a

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well-known theory of gravitational accretion. The wake material will tend to pull itself together by its self-gravitation and so form a 'daughter' galaxy. There is a critical condition for the daughter to break away from her parent instead of remaining gravitationally bound. If they separate, the process will start again with each galaxy. If they remain together, the process will be repeated with the pair, and by the production of further offspring a cluster of galaxies of increasing population may result. But a cluster has an inherent tendency to lose members by 'evaporation'; this and other factors hinder its growing indefinitely. The galaxies that 'evaporate' off maintain the distribution of random motions of the field galaxies. Sciama finds there to be a unique and stable self-perpetuating system of this sort; it has no free parameters, and all its characteristics depend ultimately upon the properties of the atomic hydrogen which constitutes the bulk of the intergalactic material postulated in the theory. Thus we explicitly find the characteristics of the universe of galaxies to depend upon the fundamental properties of the matter from which it is constructed. The deduced characteristics are compatible with those observed. So far-ranging a theory has, of course, not yet been developed in mathematical detail, and Sciama has to employ much simplified treatments of some of the steps.

Sciama's theory cannot yet be regarded as established. But its attractiveness is obvious. It has no arbitrary feature. At the same time, every known main feature of the actual galaxies plays an essential part. By contrast, any other theory that has been proposed, even if it succeeded in explaining some features such as the masses, would apparently require a more or less independent explanation of other features, such as the linear or rotational motions. Moreover, the theory automatically disposes of one criticism of steady-state cosmology, namely that this seemed to entail the indefinite growth of any particular galaxy by accretion of the postulated intergalactic matter. On Sciama's theory a galaxy is indeed always engaged in accreting this matter, but this is the very process of forming new galaxies and not of growth of the given galaxy.

INTERGALACTIC MATTER

The steady-state hypothesis demands a modification of the ordinary form of the law of conservation of matter. But the modification is vastly less than the margin of accuracy to which this, or any other 'law', has ever been verified empirically. In fact we require the continual creation of new matter at the rate expressed by

$$\frac{\text{Matter created per year}}{\text{Existing matter}} = \frac{3}{\tau} \simeq \frac{1}{2 \times 10^9} \quad \ldots . (1)$$

This means that existing matter moves out of any region as a result of cosmical expansion, and is replaced by 'new' matter, once in about 2×10^9 years on the average.

In this and the two following sections we assume that creation does occur at the required rate, and we pursue its consequences.[1]

The new matter maintains the supply of raw material for new galaxies. It follows that

(*a*) it must provide predominantly hydrogen gas;

(*b*) its creation must not be restricted to the vicinity of existing galaxies.

Hoyle's original theoretical mean density ρ for the steady-state universe was

$$\rho = (\tfrac{8}{3}\pi\gamma\tau^2)^{-1} \quad (\gamma = \text{gravitational constant}) \ldots (2)$$

and almost all other treatments reproduce this. Using

$$\tau = 6 \times 10^9 \text{ years} \simeq 2 \times 10^{17} \text{ seconds} \ldots . (3)$$

we find

$$\rho \simeq 5 \times 10^{-29} \text{ g cm}^{-3} \simeq 3 \times 10^{-5} \text{ hydrogen atom cm}^{-3} \quad \ldots . (4)$$

The amount of matter in the form of galaxies is usually estimated at an order of magnitude less than this (see [14] where (§133) a somewhat different distance-scale is used, but this will not much affect the ratio of the amounts).

These figures imply that some 90 per cent of all matter is intergalactic. The exact value of the ratio is not important for the moment; what is important is that a preponderance of intergalactic matter is admissible.

We obtain a self-consistent picture if we further infer that

(*c*) the intergalactic matter is, approximately, uniformly distributed through space;

[1] On account of the equivalence of mass and energy, continual creation may be said to involve, to the small extent stated, a modification of the first law of thermodynamics. It is this which keeps the mass or energy steady in any region. Continual creation involves a corresponding modification of the second law of thermodynamics, keeping the entropy steady in the region. But here we need not consider this further.

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(*d*) this distribution is maintained by the creation of new matter, occurring approximately uniformly through space.

These inferences are consistent both with (*b*) and with Sciama's theory. Also, because of the preponderance of intergalactic matter, they imply that

(*e*) creation occurs where most of the matter is.

Thus we need not assume that the appearance of new matter is unconnected with the presence of existing matter. All that it seems necessary to exclude is such a dependence upon existing matter as would cause a continual mass-increase of each galaxy.

PHYSICAL STATE OF INTERGALACTIC MATTER

All physical processes in intergalactic matter must be extremely slow, because of the long mean free paths. But we have seen that the matter is replaced on the average in about 2×10^9 years. It follows that processes that take longer than this to produce appreciable effects are not significant. The writer is not aware of any published theory of the physical state of intergalactic matter taking account of such considerations; this and the following section are based on his preliminary results.

The creation process must be one involving fundamental particles and producing hydrogen. Consequently at some stage it must yield protons and electrons. Neutral hydrogen atoms, not being fundamental particles, can appear only through the subsequent mutual captures of protons and electrons.

It is interesting that there is no reason to suppose the universe as a whole to be electrically neutral, and the consequences of supposing otherwise would be worth examination. For the sake of simplicity, however, we shall assume equal numbers of protons and electrons. Also we will treat only the smoothed-out universe.

Let $\mathcal{N}_p$, $\mathcal{N}_e$, $\mathcal{N}_H$ = steady-state number-density of protons, electrons, hydrogen atoms. We write

$$\mathcal{N} = \mathcal{N}_p + \mathcal{N}_H \qquad \ldots.(5)$$

Also let $\alpha\, \mathcal{N}_p\, \mathcal{N}_e$ = production rate of hydrogen atoms/unit volume and n_p, n_e = creation rate of protons, electrons/unit volume.
We are assuming

$$\mathcal{N}_p = \mathcal{N}_e \quad \text{and} \quad n_p = n_e \qquad \ldots.(6)$$

Let O be an origin at rest in the medium and consider the material in a sphere about O of fixed radius r. Owing to cosmical expansion, the material is flowing out through the spherical boundary with radial velocity r/τ.

In the steady state, the total amount of material flowing out equals the total amount created inside, and the number of hydrogen atoms flowing out equals the number produced inside. Thus

$$4\pi r^2 \mathcal{N} r/\tau = \tfrac{4}{3}\pi r^3 n_p \qquad \ldots.(7)$$

$$4\pi r^2 \mathcal{N}_H r/\tau = \tfrac{4}{3}\pi r^3 \alpha \mathcal{N}_p\, \mathcal{N}_e \qquad \ldots.(8)$$

From (7) we get the creation rate, in agreement with (1):

$$n_p = 3\mathcal{N}/\tau \qquad \ldots.(9)$$

From (5), (6), and (8) we get

$$\mathcal{N}_p^2 = \frac{3}{\alpha\tau}(\mathcal{N} - \mathcal{N}_p) \qquad \ldots.(10)$$

This is the *dissociation formula* for the material.

We assume there to be a kinetic temperature T. Also we neglect photo- and collisional ionization, as seems justifiable for intergalactic space. Then α is the usual 'recombination coefficient' for hydrogen, and we have ([14] §36)

$$\alpha \simeq 3 \times 10^{-10}\, T^{-\frac{1}{2}}\ \mathrm{cm^3\,sec^{-1}} \qquad \ldots.(11)$$

Using $\tau = 2 \times 10^{17}$ sec, $\mathcal{N} = 3 \times 10^{-5}$, as in (3) and (4) we find from (10) and (11) 90 per cent ionization at about $T = 8 \times 10^4$ degrees and 10 per cent at about $T = 12$ degrees.

Thus, over a big range of temperature, there will be significant fractions of both neutral and ionized hydrogen. Until more is known of a possible creation process, however, we cannot predict a value of T.

POSSIBILITY OF OBSERVING INTERGALACTIC MATTER

An observational determination of the contents of intergalactic space would be about the most useful possible advance in cosmology, for we have seen that these contents may form the bulk of the matter in the universe, and knowledge of their nature would help greatly in choosing between theories. Some way of making the determination will almost certainly be discovered, but unfortunately no present means is likely to succeed.

There is apparently no optical means of detecting ionized or neutral intergalactic hydrogen, for much the same reasons as those which preclude the optical determination of the general distribution of interstellar hydrogen within the Galaxy.

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As regards the possibility of detection by the 21 cm radio line, owing to the cosmical red-shift, which increases with distance, intergalactic hydrogen of the estimated density has small optical thickness at any given wavelength. Consequently any absorption effect is very small. Any emission will not produce a 'line' but a band of very small, roughly uniform strength extending on the long-wave side of 21 cm. It would almost certainly be too weak to be observed.

Among possible methods to be investigated are any that might be based upon the slight electrical conductivity imparted to intergalactic space by ionized hydrogen, or upon any effects in the medium that might be produced by the outburst of a supernova in a galaxy. Any direct observational evidence for Sciama's process of galaxy production would also be evidence for the presence of intergalactic hydrogen.

It ought to be recalled that 21 cm emission reveals [15] a tenuous envelope of neutral hydrogen around the Magellanic Cloud galaxies out to diameters two or three times those of the visible parts. This is not positive evidence for a general intergalactic medium, but it would not be surprising to find such envelopes to be continuous with a general medium. A. E. Whitford [16] has called attention to the possibility of the more general detection of intergalactic neutral hydrogen within the non-expanding Local Group.

CONNECTION WITH ATOMIC PHYSICS

The writer has shown [17] that the possibility of continual creation is not incompatible with general relativity theory. F. A. E. Pirani [18] has carried the argument a stage further by showing what type of hypothetical elementary process would achieve the required result on a formally orthodox interpretation of that theory. Further, from the standpoint of nuclear physics, A. Salam [19] has remarked that, since non-conservation of 'parity' has been discovered in nuclear interactions that are 'weak' in a specific sense, the sort of non-conservation required for continual creation might apply to interactions at the next order of 'weakness'. Thus the concept of continual creation has not proved repugnant to current physical theory.

It has, indeed, provoked a further line of speculation. For 'matter', constituted of positively charged nuclei and negative electrons, and 'anti-matter', constituted of negatively charged nuclei and positive electrons, are treated in a symmetric fashion by current theory. In particular, both sorts have theoretically identical inertial and gravitational properties. On general grounds we might therefore expect any creation process to produce statistically equal amounts of matter and anti-matter and the universe in the large to be composed of equal amounts. On the other hand, any particular galaxy must be composed effectively entirely of one sort only. G. R. Burbidge and F. Hoyle [20] have shown that a galaxy might sustain at most about 1 part in 10^7 of the opposite sort. It has even been suggested that some very energetic collisions observed to be occurring in some parts of the universe are between galaxies of opposite compositions. We should thus be confronted with the formidable problem of how matter and anti-matter, intermingled in space, could be segregated to produce galaxies.

We have already noticed, however, that new matter must appear, if at all, in the presence of existing matter. The problem just stated would not arise if one sort of matter bred only its like. Then, if the universe as a whole does contain comparable amounts of both kinds, we should conclude that very large tracts contain only one sort or the other. Or the universe might 'happen' to be composed almost entirely of the sort we call matter, with anti-matter as no more than a rarity anywhere.

The questions involved here are of general physical interest, apart from particular cosmological theories, and they would arise in much the same form in any cosmology. Interest in them was reawakened by the laboratory observation of the antiproton in 1955. Another laboratory observation, that of the neutrino in 1956, also stimulated cosmological speculation [21]. For neutrinos might pervade the universe in great numbers and contribute to the general gravitational or other fields without producing any other observable effects.

REVIEW

Hubble's law of red-shifts, the homogeneity of the universe, the existence of the time-characteristic of the universe τ, the relation between the gravitational constant, the time-characteristic, and the mean density (equation (2), which follows from almost all interpretations with only minor differences in the numerical factor)—these are all discoveries of great and probably lasting importance. Beyond these, however, our survey would seem disappointingly inconclusive were it not possible to see some significance in the apparent inconclusiveness.

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The steady-state theory has survived for the nine years since its announcement. Despite its peculiar vulnerability, no observation has been found to refute it that is not itself of uncertain interpretation. The observation most frequently quoted against it, the Stebbins-Whitford effect, has itself been refuted [8]. The obstacle regarding nuclear synthesis has disappeared and, with it, one of the main arguments for evolutionary theory. The solution of the problem of galaxy formation seems to be within reach of steady-state cosmology, while other theories have not shown much progress towards solving it.

If the universe has been evolving at the rate inferred from evolutionary theory, so that its age is about the same as that of the Earth, we should expect to find many results in astrophysics obviously to depend upon this fact. As it is, there is nothing in current astrophysics that can be claimed to require it. But if the universe is in a steady state, astrophysics must proceed without appeal to changing cosmical conditions: this is precisely how it is proceeding. In that case, moreover, it must be much more difficult to use astrophysics to infer anything about the cosmical conditions than it would be if those conditions were changing.

For these reasons, the present state of inconclusiveness is probably no worse than we might expect according to steady-state cosmology. It is actually evidence for the validity of the theory.

On the observational side, it has to be remembered that the study of the universe in the large is possible with only a few existing instruments, and that so far as radio-astronomy is concerned we have seen only the beginning of its possibilities. It seems as though an extension of existing work to not prohibitively greater distances would settle some vital issues. But difficult statistical problems are concerned in saying what extension is actually necessary. It is much to be hoped that some unexpected discovery will open the way to the study of whatever intergalactic material exists.

If we accept the evidence that creation has occurred at least once, it seems natural to consider the possibility of its occurring more than once. In fact, the concept of continual creation calls for investigation without, necessarily, special reference to any particular theory of cosmology. The matters briefly mentioned in the preceding section show that discoveries in laboratory physics are explicitly giving rise to cosmological problems, and especially to ones concerned with creation of matter. They also show the possible existence of factors that do not emerge from astronomical investigations alone, and that may account for some of the lack of progress in astronomical cosmology. Progress in cosmology may have to await further progress in physics.

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Insect viruses and their structure

KENNETH M. SMITH and ROBLEY C. WILLIAMS

Virus research has tended to be concentrated upon forms that cause disease in man or in the crops and animals on which he depends. But such practical considerations are not necessarily important in fundamental research: for this purpose certain insect viruses, not hitherto very closely studied, seem to have important possibilities which are here discussed. A virus which infects larvae of *Tipula paludosa* has particularly interesting properties, and an ingenious shadow-casting technique has made it possible to identify its particles as icosahedra: it is the first virus in which the geometrical form of the particles has been demonstrated.

Viruses and virus diseases have been investigated by people whose aims have been fairly readily distinguishable as humanitarian, economic, or fundamental. The first aim involves consideration of the virus ailments of man and their alleviation, while the second is concerned with study of the diseases of the higher plants and animals and with reducing the losses that result from these diseases. The viruses that affect insects have not received much attention from those whose approach is either humanitarian or economic, and consequently they are not well understood from this point of view. But the third approach—the fundamental study of viruses—is concerned only with the questions of what viruses are and how they multiply within living cells. Here one kind of virus is as good as another if it promises to provide significant information. In this type of study, insect viruses may be important, and some of their known properties lead us to believe that recognition of their importance in fundamental virus research is overdue.

Although viruses attack almost every known kind of organism, there seem to be some exceptions. For example, in the plant kingdom no virus has been discovered that infects the ferns or the true fungi. In the insect world only three orders are definitely known to be susceptible to virus infection. This apparent restriction in host range may, in part, be only a reflection of our lack of knowledge, but it is so far the fact that viruses have been found only in the Lepidoptera (butterflies and moths), the Hymenoptera (ants, bees, and wasps), and the Diptera (the true flies). No virus diseases have been reported for the populous orders like the Hemiptera, which includes the aphids and plant bugs; the Orthoptera, such as grasshoppers and locusts; and the Coleoptera, containing some forty thousand species of beetles.

For practical reasons it might be distinctly important to discover virus diseases in some of the insect orders not now believed to harbour them. The deliberate inducing of virus plagues among insects has been studied for some time in the hope of exercising biological control over insect pests. Such attempts involve first collecting large quantities of some particular virus and then spraying the infectious material in trial fields where the appropriate insect pest is in the susceptible stage. Considerable success in such trials has been occasionally achieved [1], with dramatic mortality among the pests and no damage to the crops. But one of the drawbacks to the general use of this form of control is the existence of several kinds of insect pests, such as aphids and locusts, that appear to have no natural virus enemies.

KINDS OF INSECT VIRUSES

Since the symptoms displayed by insects suffering from virus diseases are rather variable and non-specific, the classification of insect viruses has been based upon their morphological and cytological characteristics. The characteristics used relate either to the appearance of the virus particles themselves, as seen under the electron microscope, or to the appearance and intracellular localization of certain inclusion bodies associated with the virus particles. On the latter basis three groups of insect viruses are now recognized, although one of the groups is commonly divided into two sub-types. The virus particles associated with any one group or sub-type seem to have a characteristic size and shape. In the few cases where the particles have been purified and analysed it has been found that they are nucleoproteins, with the nucleic acid of the deoxy type [2]. The types of disease produced by the viruses of a group seem to depend upon where the virus multiplies within the body of the host. Although there are instances of virus being recovered from

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insects in the pupal, and even adult, stage [3], it is generally conceded that only the larval forms are susceptible to frank virus diseases. It is found that within the larvae many organs and types of cells are liable to infection by some virus of one of the three groups. The cells most commonly invaded are those of the skin, the tracheae, the fat-body, the mid-gut, and the circulating cells of the blood.

Polyhedroses. The largest of the three groups of insect viruses so far investigated is the one causing the polyhedral diseases, or polyhedroses. This name derives from the observation that large numbers of bodies of polyhedral shape are present within certain tissues of the infected host. In a few cases these bodies have been shown to be crystalline, as examined by X-ray analysis [4] and by electron microscopy [5]. The polyhedral crystals are not uniform in shape and size, but they are sufficiently characteristic in appearance, and large enough, to be easily recognized under the light microscope. For many years there was much controversy about the nature of the polyhedral bodies, and for some time they were generally thought to be some kind of micro-organism causing the disease. It is now known that the bodies are primarily protein crystals within which several hundred virus particles are occluded. We shall refer to true virus crystals later, and it should be noted that the polyhedral bodies, though crystalline, contain virus particles only as a minor constituent.

The polyhedral virus group is divisible into two types, in each of which the polyhedral bodies have characteristic properties and unique localization within infected cells. The nuclear polyhedral viruses usually attack the skin, tracheae, fat-body, or blood cells, and as their specific name implies, they are found to develop only in the nuclei of the cells affected. When the affected cells become grossly infected they burst and release the polyhedral bodies into the blood. These released polyhedral bodies are readily obtained in pure form by gentle sedimentation and decantation of a water suspension of the contents of the bodies of infected insects. When the polyhedra are treated with weak alkali the protein contained in them dissolves, leaving behind what appears to be an outer membrane within which are rod-shaped virus particles. They may be found either singly or in bundles (figure 1). It is possible to obtain the rod-like particles quite pure, and when this is done they are found to be highly infectious when inoculated into healthy larvae [6].

The cytoplasmic polyhedroses have been found only in the cells of the gut, usually the hind-gut, where they seem to develop solely in the cytoplasm of the affected cells. The polyhedral bodies may be extracted from the cells and purified; when this is done, the purified material proves to be infectious [7]. The morphological identification of the elementary infectious particle is not, however, as clear-cut as it is in the case of the nuclear polyhedroses. When the cytoplasmic polyhedral bodies are treated with alkali they partially dissolve, leaving behind a sort of matrix honeycombed with round holes, and a number of small, spherical particles [8]. Since these particles have not been assayed for infectivity it cannot be stated with assurance that they are the elementary infectious agent. The electron microscope reveals in thin sections of the cytoplasmic polyhedra the presence of small, approximately circular particles (figure 2) that are probably of the same nature as those extractable by weak alkali [9]. Inasmuch as sections cut through nuclear polyhedra show characteristic particles known to be viral in nature, it can be postulated, but only by analogy, that the small circular objects seen in sections of cytoplasmic polyhedra are the virus particles.

The two types of polyhedral virus disease differ in the symptoms they produce, particularly in lepidopterous larvae. The nuclear polyhedral viruses usually attack the skin, rendering it extremely thin and fragile; it ruptures at a touch and liberates the liquefied contents of the body, which consist largely of polyhedral crystals. Caterpillars that have died from a nuclear polyhedrosis are usually found in a characteristic position hanging by one or two pairs of the abdominal 'feet'. Infected caterpillars of the nun moth (*Lymantria monacha*) tend to migrate to the tops of spruce trees (their usual habitat), where they remain hanging head downwards. This rather peculiar behaviour has given rise to the German name of *Wipfelkrankheit*, or tree-top disease. In the disease produced by the cytoplasmic polyhedroses the skin is not attacked. The caterpillar dies intact and tends to dry up rather than liquefy. Towards the termination of the disease, in some cases, the cells of the gut are crammed with polyhedra and finally burst, spilling out the contents to be voided with the faeces. Frequently it is possible to see the polyhedra showing through the skin as white patches.

Granuloses. A second group of insect viruses is responsible for the diseases known as 'granuloses', a name deriving from the fact that the elementary virus particles appear to be enclosed in a

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FIGURE 1 – *Nuclear polyhedral body from* Bombyx mori, *dissolved to disclose virus particles. Many are still enclosed within a membrane.* (× 20 000)

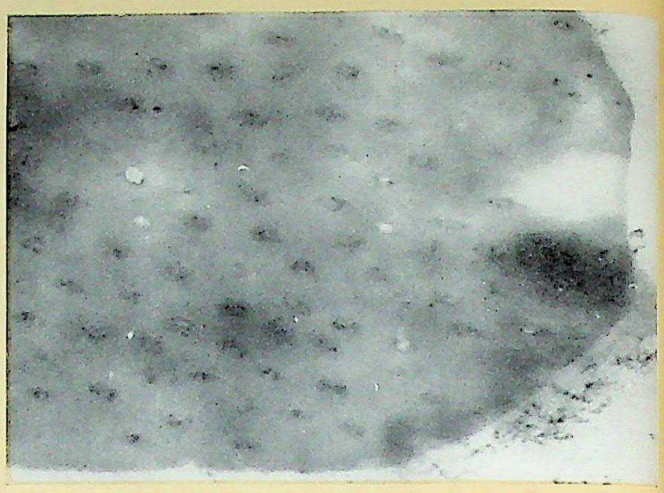

FIGURE 2 – *Section of cytoplasmic polyhedral body from* Arctia caja. *The quasi-circular bodies within are presumed to be sectioned virus particles.* (× 85 000)

FIGURE 3 – '*Granules*' *from a granulosis disease of* Euplexia leucipera *after treatment with alkali. Note central capsules and flattened granules. The virus particle is inside the central capsule.* (× 26 000)

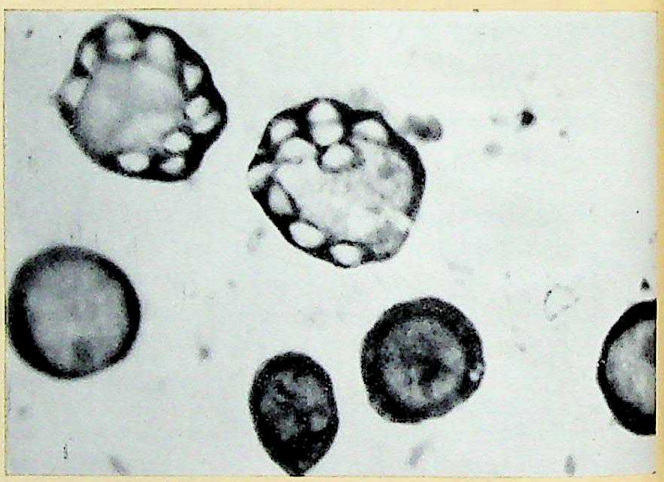

FIGURE 4 – *Blood cells of* T. paludosa *infected with a nuclear polyhedrosis. Upper two cells contain many polyhedral bodies. Light micrograph.* (× 1500)

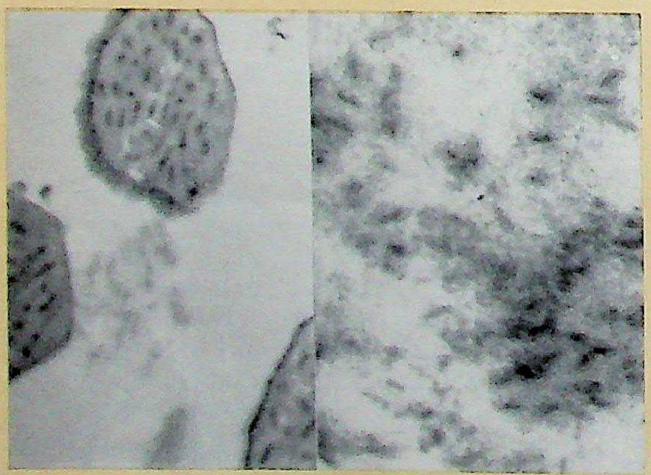

FIGURE 5 – (*left*) *Sections of polyhedral bodies from* T. paludosa, *containing rod-shaped particles* (× 24 000). (*right*) *Portion of nucleus showing similar particles, not enclosed in polyhedral body* (× 30 000).

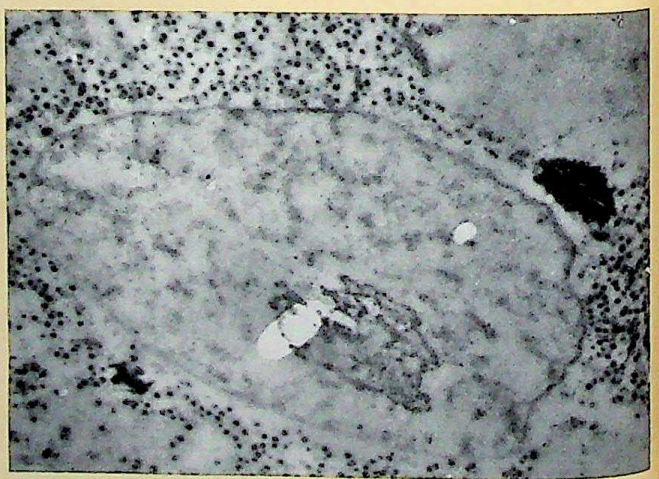

FIGURE 6 – *Portion of fat-body cell infected with* Tipula *iridescent virus (TIV). The darkly staining virus particles are solely in the cytoplasm.* (× 8500)

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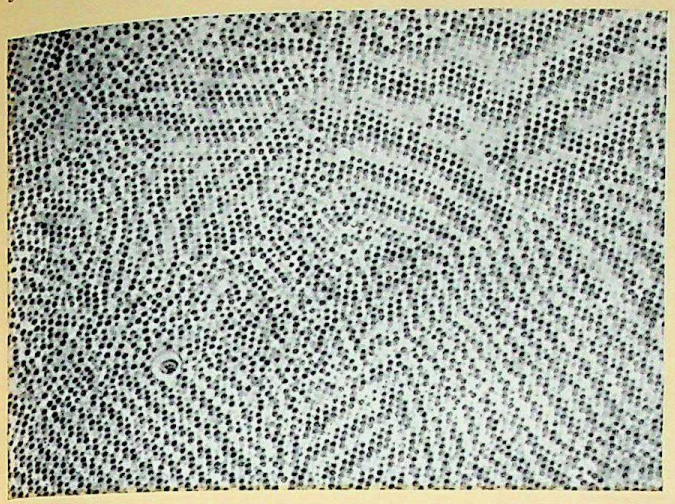

FIGURE 7 – *Pellet of TIV sectioned to show the existence of a polycrystalline array.* (× 10 000)

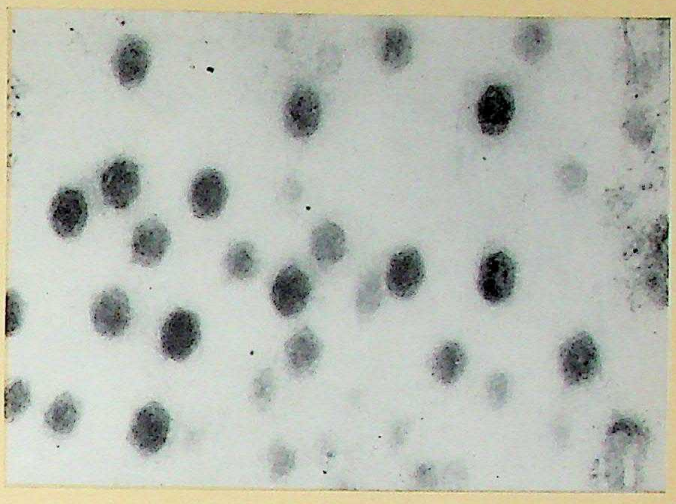

FIGURE 8 – *Sectioned particles of TIV, exhibiting some internal structure and frequent six-sided forms.* (× 55 000)

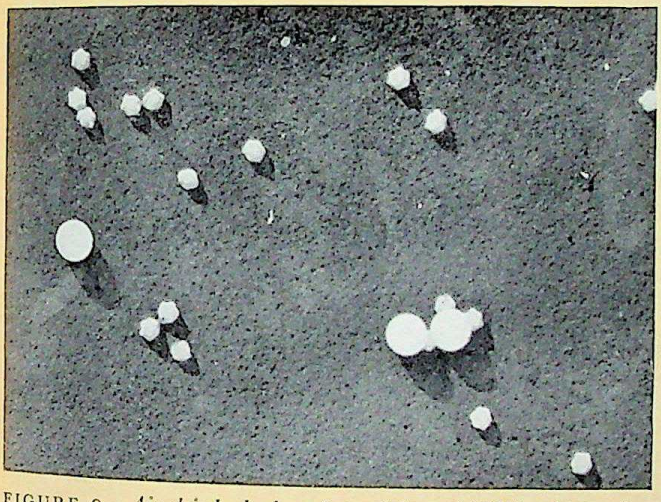

FIGURE 9 – *Air-dried shadowed particles of TIV. Note six-sided contours. Spherical objects are polystyrene latex particles for reference.* (× 23 000)

FIGURE 10 – *Two frozen-dried particles of TIV, showing that two distinct shadow forms may be cast: four-sided and pointed, five-sided and blunt.* (× 67 000)

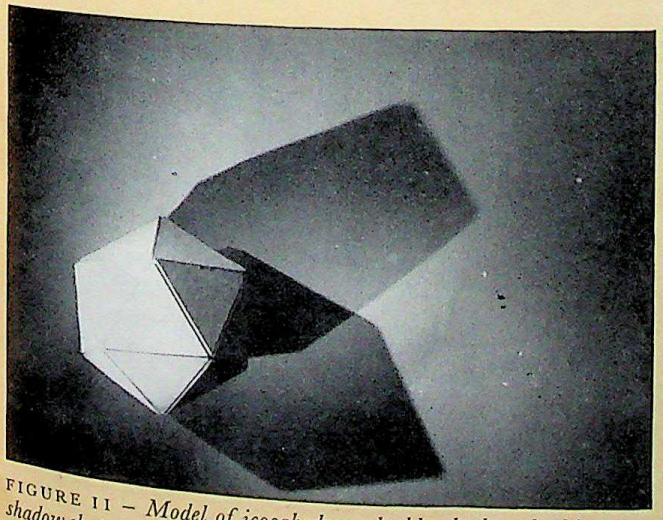

FIGURE 11 – *Model of icosahedron, doubly shadowed. Note two shadow shapes which can be cast simultaneously only by an icosahedron in this orientation.*

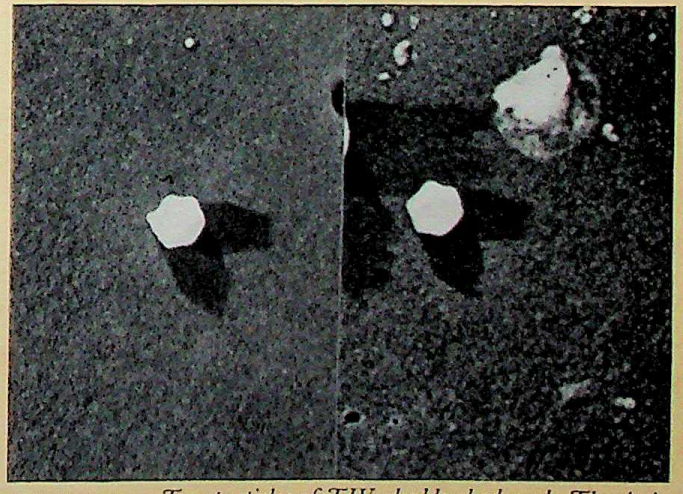

FIGURE 12 – *Two particles of TIV, doubly shadowed. The similarity between the shapes of these shadows and those of the model (figure 11) is evident.* (× 55 000)

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FIGURE 13 – *Two* T. paludosa *larvae severely infected with TIV. The normal tan colour has changed to a bluish grey.*

FIGURE 14 – *A* T. paludosa *larva, infected with TIV, dissected to show the blue colour of the hypertrophied fat-body.*

FIGURE 15 – *A bottle of purified TIV that has stood in the cold. Note that a layer of coloured crystallites has formed at the bottom.*

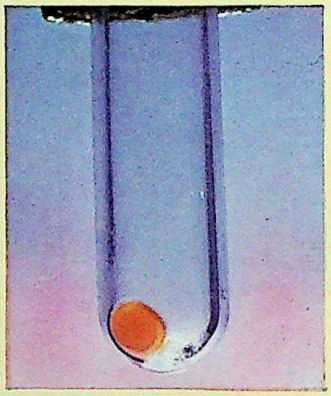

FIGURE 16 – *A centrifuged pellet of purified TIV photographed by transmitted light. The pellet appears uniformly orange.*

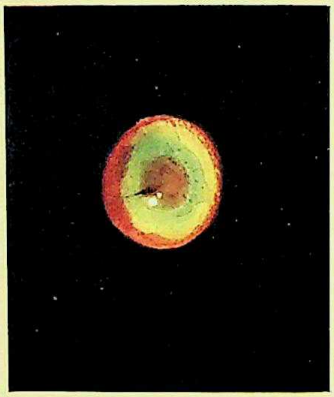

FIGURE 17 – *A pellet of TIV allowed to re-hydrate and photographed by reflected white light. The iridescence is evident.*

FIGURE 18 – *An array of crystals of TIV photographed by reflected white light.* (× 10)

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capsule or granule. No polyhedral bodies are found in cells infected with a granulosis disease. That the encapsulated particles are the causative agents of the granuloses is established from the observation that a suspension of purified granules is highly infectious [10]. When the granules are treated with weak alkali a rather complex structure is disclosed. There is an outer capsule shaped like a prolate spheroid, and inside this there is found a similarly shaped, but separable, capsule (figure 3). Within this inner structure a rod-shaped particle is found, apparently enclosed in a limiting membrane [11]. It is not known if the innermost particle, with its intimate membrane, is infectious.

The granulosis group of viruses has so far been found to be restricted in host range to the caterpillars of Lepidoptera. Examination of thin sections of tissues of infected insects reveals bodies that are similar in shape and size to sectioned granules, and from such observations an indication can be obtained of the likely sites of multiplication of the virus [12]. The sites are chiefly the tissues of the skin and the fat-body. The progress of the disease resembles that induced by the nuclear polyhedroses, in that the body contents are liquefied and are easily liberated by rupture of the skin.

The granulosis viruses may have a practical importance because some of their hosts—like the caterpillars of the white, or cabbage, butterfly—are insects of economic importance. Since a suspension of highly infectious granulosis virus can be produced in large quantity (as much as one gallon from five infected larvae), it is likely that biological control of these lepidopterous pests can be secured through spraying infested fields.

Non-encapsulated viruses. A third group of insect virus diseases is characterized by the occurrence of infectious particles generally similar in appearance to those found in virus diseases of animals and higher plants. The virus particles are not associated with any kind of inclusion crystal or encapsulation, but rather are found free in the cells of the particular tissue attacked. Only two examples [13, 14] of this group are so far definitely known, one occurring in larvae of Diptera, the other in larvae of Lepidoptera. It is possible to extract the particles from the cells and purify them; in both cases the purified, approximately spherical, particles have been found to be infectious. One member of this group of non-encapsulated viruses, here called the *Tipula* iridescent virus (TIV) is of considerable interest, and will be described in some detail later in this article.

HOW INSECT VIRUSES ARE SPREAD

Many of the plant viruses, and some of the viruses affecting higher animals, are transferred from host to host only by means of arthropod vectors, such as aphids, mosquitos, and mites. Insect viruses are mostly, if not wholly, spread from diseased to healthy larvae by the ingestion of infected food. The efficiency of spreading is greatly increased by the way in which the virus particles are, as it were, often 'packaged for delivery', i.e. contained in granules and polyhedral bodies which may retain their infectivity for several years. On the other hand, the particles of the nuclear polyhedral viruses are quite labile after removal from their protein matrix. The manner of development of the nuclear polyhedroses is also favourable towards the spread of disease; the skin of the diseased insect ruptures on death and the liquefied contents of the body get splashed by rain upon plants used for food by healthy larvae. In addition, in some cases the healthy larvae are strongly attracted by the liquefying cadavers, on which they feed with devastating results.

The way in which the cytoplasmic polyhedroses develop, however, is not so favourable to the spread of these diseases. Caterpillars infected with this type of polyhedrosis tend to dry up rather than liquefy, and on death they retain their body structures intact. It is possibly for this reason that these diseases are found to spread more slowly than do the nuclear polyhedroses. The method of spread of the cytoplasmic polyhedra is probably by ingestion of faeces contaminated in the late stages of the disease by polyhedra liberated into the lumen of the gut.

There is very little information about how the viruses that lack inclusion bodies are spread, but presumably they are in some manner ingested with the food. It is a little difficult to visualize how the *Tipula* iridescent virus gets about, since its specific host, the larva of *Tipula paludosa* (the crane fly or daddy-long-legs) is a soil-inhabiting insect. No doubt in this case also the food becomes contaminated, but the comparative rarity of this virus disease may perhaps be accounted for by difficulty in dissemination.

There is good evidence that some of the insect viruses are passed through the egg to succeeding generations. Examples of this type of passage are found in a cytoplasmic polyhedral virus from *Arctia caja* (the garden tiger moth) [15], and in a

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granulosis virus affecting the caterpillars of *Pieris brassicae* (the large white butterfly) [16]. This mode of dissemination of disease is of practical importance in some cases where biological control of insect pests is being attempted, since passage of virus through the egg allows us to infect future generations, which is not possible with insecticides. The phenomenon of the passage of a virus to the progeny of infected insects brings us to the consideration of 'latency' in insect viruses.

LATENT VIRUSES

Many examples are known of viruses that may remain dormant in their hosts for prolonged periods, even through successive generations, and that suddenly begin to cause pathological changes in the absence of any overt inoculation of the host. In their dormant stage such viruses are said to be 'latent', an indefinite term indicative of our ignorance of what is happening. Latency is found throughout the virus diseases of bacteria, plants (where it is quite common), the higher animals, and insects. It is encountered with great frequency in the virus diseases of insects, where in some populations almost every individual appears to carry a latent virus. Needless to say, the ubiquitous presence of virus latency among insects renders the precise biological assay of their viruses almost impossible.

It is sometimes possible to cause a latent virus to become virulent by artificial means, such as by rearing under unsuitable environmental conditions and by feeding with chemicals. One of the most successful methods is to introduce into the insect a virus that may in the event prove to be different from the one being carried in the latent form. It has frequently happened during cross-inoculation studies that an insect develops a virus disease different from the one with which it was inoculated. Inoculation with a nuclear polyhedral virus frequently stimulates the development of a cytoplasmic polyhedrosis; but, oddly enough, the reverse seems never to occur. Once a latent virus has been transformed into a virulent one it can be transmitted from insect to insect. This circumstance may have a practical application in some cases: an experience with the larvae of the winter moth, *Operophtera brumata*, provides an example [16]. After a search for a naturally occurring virus affecting this species had failed, some apparently healthy larvae were inoculated with a nuclear polyhedral virus from the caterpillar of a butterfly, *Vanessa cardui*. A very high mortality resulted, all the larvae dying, but they succumbed to a cytoplasmic polyhedrosis. Once this virus had been established in virulent form it was transmitted successfully to numbers of winter moth larvae, allowing a stock of virus suspension to be built up for use in experiments on biological control.

TWO INTERESTING INSECT VIRUSES

Although very few virus diseases have so far been found in the larvae of Diptera (flies), two have such interesting particles associated with them that they merit a detailed description. Both diseases affect the larvae (popularly known as leatherjackets) of *Tipula paludosa*, and both may occur simultaneously in the same insect. One virus causes a polyhedral disease of a type unusual in that the associated polyhedra are not soluble in the usual reagents [17]. Affected larvae exhibit a characteristic pallor that is apparently due to an abnormally large number of blood cells having nuclei largely filled with polyhedral bodies. The virus seems to multiply within the greatly enlarged nucleus of the blood cell; as the disease progresses, the polyhedral bodies begin to form around the periphery of the nucleus in close proximity to the nuclear membrane (figure 4).

As has been mentioned earlier, nuclear polyhedra usually dissolve readily in weak alkali, leaving behind a thin membrane containing a number of virus rods. The behaviour of the nuclear polyhedra of *T. paludosa* towards different reagents is unusual, however, and differentiates them sharply from the other known types of nuclear polyhedra. They are resistant to trypsin, and to dilute and weak acids and alkalis. In molar sodium hydroxide, however, they elongate to several times their normal length, becoming first biconvex spindles and then extending into crescents or worm-like shapes. At about three times their normal length the elongation is still completely reversible, and in water at *p*H 5–8 they return to their original size and shape. The elongation and the return to normal length (which takes place along the same axis) can be repeated indefinitely and as rapidly as the necessary solutions can be alternated. The failure of the polyhedral bodies to dissolve makes it impossible to see by electron microscopy any virus particles contained within. It is necessary to examine thin sections of the polyhedra in order to see what is inside them. In the electron microscope the sections reveal the presence of rod-shaped particles, presumed to be the elementary virus particles. Figure 5 shows some such sections, some apparently longitudinal and some transverse.

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Larvae of *T. paludosa* collected from the field are occasionally found to be harbouring a virus disease easily recognizable by the change in appearance that it induces in the body of the affected host [14, 18, 19]. Whereas the normal appearance of these larvae is a dark tan, the colour of the diseased ones is a somewhat opalescent blue-indigo (figure 13). Examination by low-power microscopy shows that the colour is particularly intense in the lobes of the hypertrophied fat-body (figure 14); as the disease approaches its termination the colour is less intense and is more generally dispersed throughout the body. When sections of diseased fat-body tissue are examined in the electron microscope it is seen that the cells are filled, but only in the cytoplasm, with darkly staining particles (figure 6), believed to be the elementary virus particles. There is no indication of encapsulation around the virus particles, nor is there any sign of the development of a polyhedral body, such as would be seen in a cytoplasmic polyhedrosis.

It is quite easy to extract and purify the elementary particles from the diseased insects. Immediately after death has ensued the cadavers are simply placed in water, where they are allowed to stay for a few days while the particles leach out. A brief, low-speed centrifuging is sufficient to remove the gross material, after which a high-speed centrifuging will convert the virus particles into a pellet of fairly pure material. A repetition of the low- and the high-speed centrifuging will result in a preparation that is demonstrably homogeneous and is presumably pure. When this material is fed to healthy larvae there is an incidence of the disease distinctly higher than that found in the wild population: from this observation it may be concluded that the particles in the preparation are the causative agent of the disease. The amount of virus produced by one larva is remarkably great: measurements so far made indicate that in the late stage of the disease about one-fourth of the larva's dry body-weight will have been converted to virus particles. This figure is not approached by any other known animal virus disease, although it is approximated by the tobacco mosaic virus disease and by a few of the virus diseases of bacteria.

Pellets formed from purified TIV are found to have fascinating optical properties. When observed by transmitted light they are generally orange in colour (figure 16), not greatly different from the colour exhibited by pellets of other viruses. But when examined by reflected white light the pellets appear exquisitely iridescent, with the colours most noticeably in the blue and green regions of the spectrum. The predominant colour is related to the degree of hydration of the pellet; very 'wet' pellets are more yellow and orange (figure 17), while 'dry' ones are characteristically violet. The origin of the colour is disclosed by examination of thin sections cut through pellets that have been dehydrated and embedded for sectioning. A typical section will show a somewhat bizarre pattern of particle array, such as is shown in figure 7. As is readily seen, this pattern is what would be expected if a thin slice were made through an array of randomly orientated microcrystals. From this kind of observation it is concluded that the pellet is a mass of small crystals, each crystal being of the order of 10 microns across. In fact, the composition of a pellet can be thought of as a scaled-up model of a polycrystalline metal, such as a piece of cast alloy. It is well known that a specimen of a polycrystalline metal will exhibit Bragg reflections when illuminated with X-rays. The conditions for such reflections are that the interatomic spacings in the crystals be about half the wavelength of the X-rays employed, and that the orientation of a crystal with respect to the incident X-ray beam be such that the scattered, or 'reflected', X-rays experience constructive interference. If we were to build our crystal from large-size virus particles instead of from atoms, and substitute light waves for X-rays, we should have the proper situation for the exhibition of Bragg reflections in the visible region of the spectrum. Since we illuminate with light of all wavelengths (white light), each virus crystal will be oriented in a manner such as to reflect constructively some particular wavelength, and the overall effect will be iridescence—the reflection of light of differing colours by multiple, tiny regions. Pellets exhibiting these optical properties have not been seen for any other virus, owing to failure to satisfy the condition that the optical path-length between adjacent planes in the crystals be as large as half the wavelength of visible light. But the particles of TIV are so large that the inter-particle spacing is about 1500 Å, and, assuming a refractive index of about 1·5, the optical path-length is about 2300 Å. Hence constructive interference should occur with light of blue-green colour. Other animal viruses are known that are even larger than TIV, but they are apparently not sufficiently uniform in size and shape to crystallize.

Purified preparations of TIV have a tendency to crystallize when allowed to remain undisturbed

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in the cold, although their behaviour in this respect is not entirely predictable. A suspension of TIV in an ordinary glass tube will generally be found to have crystallized in part, with a mass of small, brilliantly reflecting crystals at the bottom and a diminished concentration of virus in the supernatant fluid (figure 15). On one occasion it was possible to form an array of crystals on the flat surface of a microscope slide, in which position they can readily be photographed (figure 18). Attempts made so far to induce crystallization by the addition of half-saturated ammonium sulphate (the classical method of crystallizing plant viruses) have been only moderately successful. Since it would be of considerable interest to examine such crystals by X-ray and optical analysis, the conditions necessary for their formation, and their stability when formed, must be further investigated.

Since the particles of TIV may be obtained obviously quite pure and in considerable quantity, they are fit subjects for chemical and physical investigations designed to give a better understanding of the structure of viruses. Only preliminary chemical analysis has so far been undertaken, and from this it appears that the virus particles consist of nucleoprotein. The nucleic acid portion is all of the deoxy type and constitutes about 15 per cent of the mass of the virus. A determination of the proportions of basic substances indicates that these are distributed much as in other insect viruses analysed. Preliminary sedimentation studies show that the virus particles form a sediment with a sharp boundary—a result not surprising in view of the high degree of morphological homogeneity, indicated by the evidence of crystallization and by electron microscopy.

The morphology of the particles of TIV may be examined by electron microscopy of thin sections of material containing them and of preparations of purified virus. Micrographs of sections show two characteristic features (figure 8), one expected and one new. The expected observation is that the particles possess to some degree a non-uniform internal structure. There appears to be an outer envelope, within which is a relatively transparent region; the central area of the particle is filled with opaque material, tentatively identified with its nucleic acid portion. Such differentiation of apparent structure is commonly encountered in sections of the larger animal viruses. The unexpected observation is that six-sided contours fairly frequently occur when the virus particles are seen in section. Regular hexagons are not seen, presumably because of distortion of the sectioned material by the microtome knife, but the relation of the direction of the apparent distortion to the known direction of cutting is such as to imply that the true contours are hexagonal.

Purified preparations of TIV appear quite monodisperse in the electron microscope, each virus particle having a diameter of about 1300 Å. Even in dry specimens prepared from a water suspension in the usual way, and hence suffering the distortions brought about by surface tension, the characteristic contour of the particles is six-sided (figure 9) rather than circular. This appearance is unique among the known viruses of comparable size: vaccinia, for example, is brick-shaped and non-uniform in size, while influenza appears quite circular and heterodisperse.

Since the particles of TIV appear to have six sides when seen in contour, it is reasonable to suppose that they are polyhedral in shape. Other examples of polyhedral-shaped virus particles are known [20] among the bacterial and plant viruses. But in no case has it hitherto been possible to arrive at a convincing notion of the exact form of the polyhedron. The difficulty has been mainly due to the smallness of the particles, making a direct determination of the shapes of the polyhedral facets uncertain. An indirect approach is to infer the three-dimensional shape of polyhedra from the shapes of the shadows formed by application of a shadow-casting technique [21]. With the small viruses this method is unreliable, owing to the relative roughness of the surface of the substrate film (cellulose nitrate) upon which the virus particles must be mounted [22].

The particles of TIV are large enough, and their shape is regular enough, to offer some hope that their full polyhedral shape can be determined by the analysis of shadow shapes. A direct observation of the shapes of the surface facets cannot be undertaken, owing to the great electron opacity of such large objects. In order to attempt shadow analysis it is essential to preserve the three-dimensional shape of the virus particles as they are dried for electron microscopy. This preservation is readily and effectively secured by freeze-drying [23]. As can be seen (figure 10), shadows of particles so prepared are distinctive in shape, and their outlines are not obscured by roughness of the substrate film.

Only three kinds of polyhedra will have six-sided contours when the object is lying upon any one of its facets. These are the octahedron, the rhombic dodecahedron, and the icosahedron. But

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as can be seen by trials with models, the octahedron will not cast a shadow having five sides, a type of shadow frequently cast by the frozen-dried particles of TIV (figure 10). The decision, then, lies in a choice between the rhombic dodecahedron and the icosahedron. The correct answer may be found by the application of double shadowing, in which two shadows are cast with azimuth angles 60° apart. In this way a single particle can, as it were, be examined from two points of view. A cardboard model of an icosahedron, illuminated by two light sources placed 60° apart relative to the model, is shown in figure 11. These points are to be noted: when the orientation of the model is such that one point of the hexagonal contour is directed toward one shadow, and the adjacent point is directed toward the other shadow, one of the two shadows formed will be five-sided and with a blunt end, while the other is four-sided and is pointed. A rhombic dodecahedron will not cast a pair of similarly shaped shadows when similarly illuminated. In figure 12 are shown two frozen-dried particles of TIV that have been double-shadowed to test the aptness of the icosahedral model. As can be seen, the correlation in shadow shapes is good. We can therefore conclude that the three-dimensional morphology of one virus is now known and that its shape is that of an icosahedron. What bearing this determination will have upon furthering our notions of virus structure remains to be seen.

ACKNOWLEDGMENTS

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The early history of the permanent magnet

E. N. DA C. ANDRADE

The history of magnetism has been but little explored, and it is not at all generally known how powerful were the magnets produced more than two centuries ago or what methods were used for their production. It will be a surprise to many, too, that permanent magnets made from powdered iron oxide—supposedly a quite recent invention—were known in the eighteenth century. This article traces the history of permanent magnets from ancient times up to the invention of the Heusler alloys at the beginning of the present century.

The manufacture of permanent magnets is today a flourishing activity, at once a science and an art, especially at Sheffield. This may cause some surprise, since there appears to be a widespread impression that such magnets have, for all important purposes, been superseded by the electromagnet. However, the permanent magnet plays an essential part in high-tension magnetos, in telephone generators, in telephone receivers, in gramophone pick-up units, in moving-coil loudspeakers, in television focus units and other equipment involving deflection of electron beams, in electricity meters, and in thermostats. It is essential also in many less familiar devices, such as permanent-magnet chucks and magnetic separators, designed to remove unwanted ferruginous particles. Much admirable research and much ingenuity have been, and are being, devoted to the special alloys now generally used, to the methods of magnetization, to shape and general design, including that of soft-iron pole-pieces, and to other highly specialized problems. Even in the field of atomic physics the permanent magnet may be found. On account of the constancy of its field, C. D. Ellis used such a magnet for work on β-ray spectra, the strength being changed by currents applied between the experiments. Large permanent magnets have been used in cosmic ray research and in radio-frequency spectroscopy. In a recent book P. R. Bardell [1] gives a table of some applications of permanent magnets notable for the variety it exhibits.

All these uses belong substantially to the present century—in fact the main developments of the permanent magnet as now known are matters of the last thirty years. In the early days of physics, however, the permanent magnet was the subject of much attention and research, especially before the electromagnet was developed and electric current supply became general. In the histories of physics the pioneering work of William Gilbert on magnetism is recorded, but the interest in the permanent magnet of such men as Canton, Aepinus, Coulomb, and Jamin, cited for other achievements, is neglected, and so are many very interesting achievements of less famous men. Some account will here be given of work up to the end of the nineteenth century, which sees the close of an epoch.

The only permanent magnet known to the ancients was the loadstone or lodestone (literally, 'way stone', from its guiding the mariner on his way; *cf.* lodestar). This is a variety of magnetite, Fe_3O_4, which as found in the natural state is magnetic. It received its classical name *magnes* because it was found in Magnesia, a district in Thessaly. Thales of Miletus (*c.* 600 B.C.) is said to have known of its abnormal properties, and its attractive powers are mentioned by various of the great Greek philosophers of the period 400–200 B.C. Later Lucretius, in *De natura rerum* (vi, 1042), records repulsion, without any very precise notion of what was in question—'Sometimes too it happens that the nature of iron is repelled from this stone, being in the habit of flying from and following it in turns.'

The first artificial permanent magnets were iron needles which had been 'touched' by a loadstone. Much has been written as to when and where the process originated. Round about 1200 the French troubadour Guyot de Provins (whom E. Hoppe [2] calls Hugue de Bercy) records in his satirical poem *La Bible* a compass made of such a touched needle supported by a floating straw. Peter Peregrinus in his famous *Epistola ad Sygerum de Foucaucourt militem de magnete*, 1269, wrote that it was known to all who had tried it that an oblong piece of iron which a loadstone had touched turned towards the pole if floated on a piece of

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FIGURE 1 – *Gilbert's capped or 'armed' loadstones, as represented in the* De Magnete.

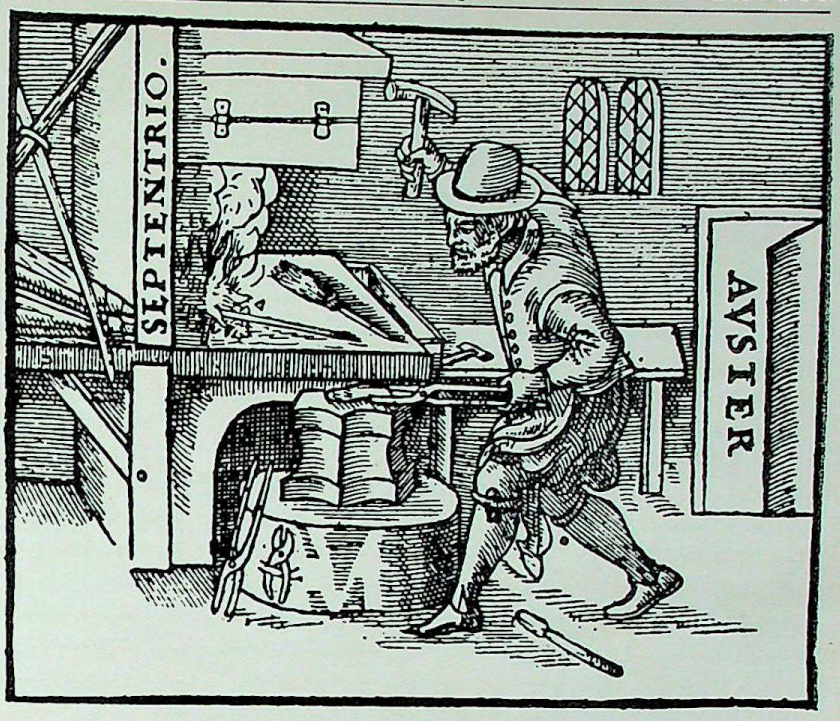

FIGURE 3 – *Smithing an iron rod in the direction of the Earth's field, as shown in Gilbert's* De Magnete. (Septentrio, *north;* auster, *south.*)

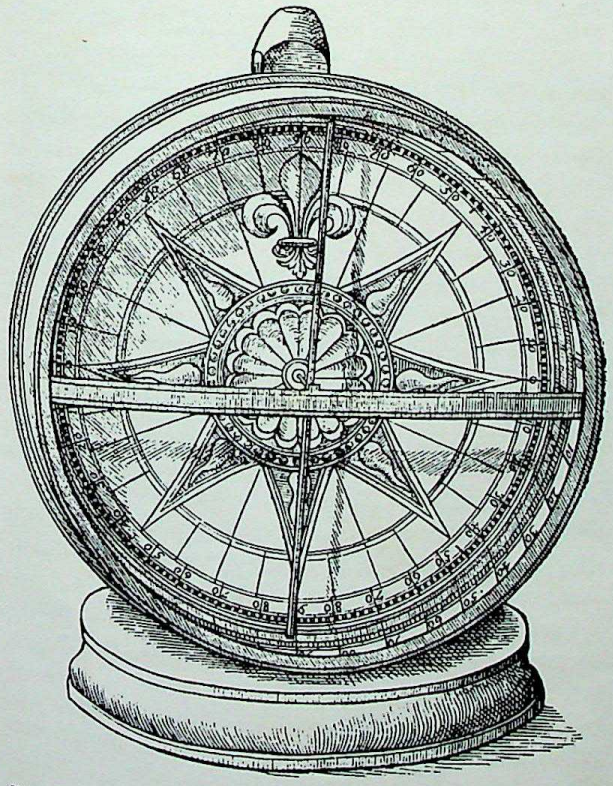

FIGURE 2 – *Gilbert's dip circle, as represented in the* De Magnete.

wood. Robert Norman in his little book 'The Newe Attractive', 1581, likewise speaks of his needles as touched with the stone, but gives no particulars. There are Chinese claims, too, but our concern here is the development of the permanent magnet, as it can be continuously followed, and these few words of reference are intended to do no more than to indicate dates of general knowledge.

The first great systematic work on the magnet is that described by William Gilbert (sometimes spelt Gilberd by contemporaries) in the famous *De Magnete Magneticisque Corporibus et de Magno Magnete Tellure Physiologia Nova*, published in 1600. This is one of the great classics of experimental physics, and its value was at once recognized by, for instance, Galileo, who wrote [3]: 'I extremely praise, admire and envy this author for that a conceit so stupendous should come into his mind, touching a thing handled by infinite sublime wits and hit upon by none of them.' In an age when much that was written about the magnet was worthless tradition and absurd, if amusing, superstition Gilbert stands out as a most careful and critical experimenter. Characteristically, he tells the reader 'Let whosoever would make the same experiments handle the bodies not heedlessly and clumsily but carefully, skilfully and deftly; when an experiment fails he should not ignorantly doubt

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our discoveries, for there is naught in these books that has not been investigated and done again and again by us.' An example of the traditional beliefs refuted by Gilbert is that the loadstone lost its attractive power in the presence of, among various other things (including garlic and goats' blood), the diamond: he showed that surrounding the stone with seventy-five diamonds had no effect. Some of his most important work was on terrestrial magnetism, which does not here concern us; but he also carried out fundamental investigations on the magnet itself.

The most powerful magnets of Gilbert's time were still loadstones. He described how to make them more powerful attractors of iron by 'arming' them, that is, by attaching soft iron caps at each end, as shown in figure 1. This is the first example of the soft iron pole-pieces used in many cases with modern permanent magnets. With a particular stone, Gilbert writes that arming increased the weight of iron that could be lifted from 4 ounces to 20 ounces; but he is careful to state that arming does not increase the attraction at a distance.

Gilbert was much concerned with making permanent magnets, or artificial magnets as they were long called to distinguish them from the natural loadstone. He needed these for compasses and dip circles, and called such pivoted needles *versoria*, which we may translate 'turnabouts', the word *versorium* being apparently of his invention. He lays stress on the material to be used: at one place he says that the needles are to be made *ex optimo aciario*, which would seem to mean of the best edge-steel, the word, not classical Latin, being derived from *acies*, an edge, in particular a sword edge (*cf.* French *acier*). Elsewhere he specifies 'the best iron which has been melted down' (*ferrum excoctum*) or 'iron made hard', so that he clearly meant to warn against the use of soft iron. As regards the size of his magnets, he said that his dip circle, shown in figure 2, might be 10–12 inches across, and he speaks elsewhere of a magnet as thick as a goose quill and about 8 inches long.

Gilbert gives three ways by which permanent magnetism (or 'verticity' as he termed it) may be given to steel. The first method was by touch with a single loadstone, which was to be drawn from the middle of the needle to the end, where the application was to be continued by a very gentle rubbing. In discussing the dip needle he speaks of touching the opposite ends with the two different poles of the loadstone, but he never deals with anything but single touch. The second method was forging with the horizontal specimen pointing north and south in the Earth's field, as shown in figure 3. He also recorded that iron wires become magnetic if drawn in a north–south direction, but not if drawn in an east–west direction. This second method, then, was one of considerable plastic deformation in the Earth's field.

Gilbert was quite clear that a red heat destroyed permanent magnetism, but records that a red-hot iron bar left to cool in the direction of the Earth's field became permanently magnetic, and further that unheated iron bars left in the direction of the Earth's field for twenty or more years—such as window bars—likewise acquired verticity. This magnetization by long exposure to the Earth's field without plastic deformation constitutes a third procedure, and these three general methods cover all that were used until the electromagnet was introduced in 1825. If J. Robison's [4] words in 1822, 'It is not saying too much of this work of Dr Gilbert's to affirm, that it contains almost everything that we know about magnetism', were an exaggeration, they were not a very great exaggeration.

Not much advance was made in the seventeenth century. The method of arming loadstones was improved by squaring the ends and applying suitably shaped iron plates, as illustrated in figure 6, which increased the attractive force on iron in contact. A typical mounted loadstone of the seventeenth century is shown in figure 4. Boyle, in 1675, shrewdly observed that the destruction of magnetism by heat without any apparent change in the metal showed that magnetism was a matter of the 'disposition or internal constitution' of the iron, and, in confirmation of Gilbert's general discovery, said that iron fire tongs and such-like that were heated and stood upright—which means nearly in the direction of the Earth's magnetic force in England—to cool sometimes became strongly magnetic. This fact was, as will be seen, applied later.

It must be remembered that loadstones were very expensive. Hartsoeker [5], in 1711, complained that he was asked 3000 guilders for one half as big as his fist, a sum which was then equivalent to about £250, and so to something like £3000 of today. The first man to make strong permanent magnets was Servington Savery [6], who described his processes in 1730. He started with wires about 1/20th inch in diameter and $2\frac{3}{4}$ inches long, of steel which he 'seasoned very hard'. These he magnetized by touch with a loadstone, but he gives no details as to method. Of 37 such wires he made a hexagonal bundle, like

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FIGURE 4 – *Stewart silver-mounted loadstone* (circa 1660).

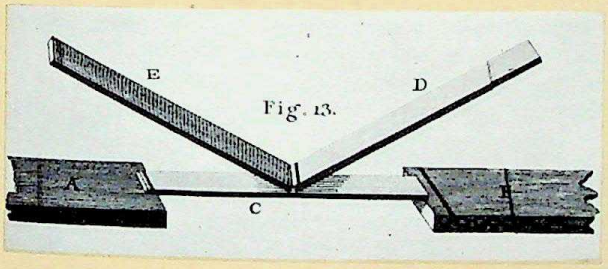

FIGURE 5 – *George Adams's representation of one of Gowin Knight's methods of magnetization.*

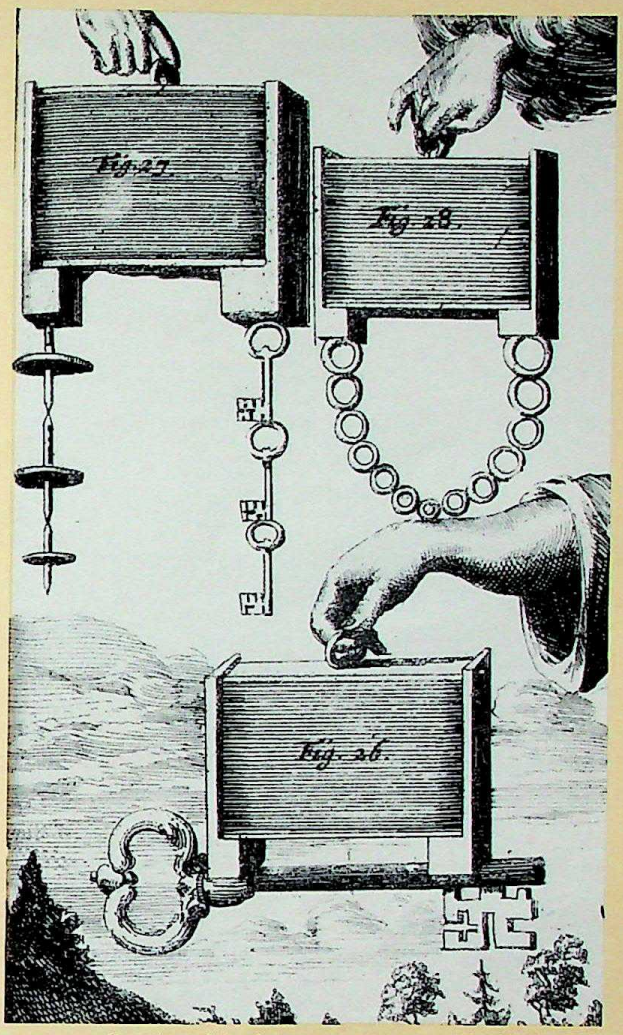

FIGURE 6 – *Armed loadstones as represented by Dalencé in his* Traitté de l'Aiman, 1687.

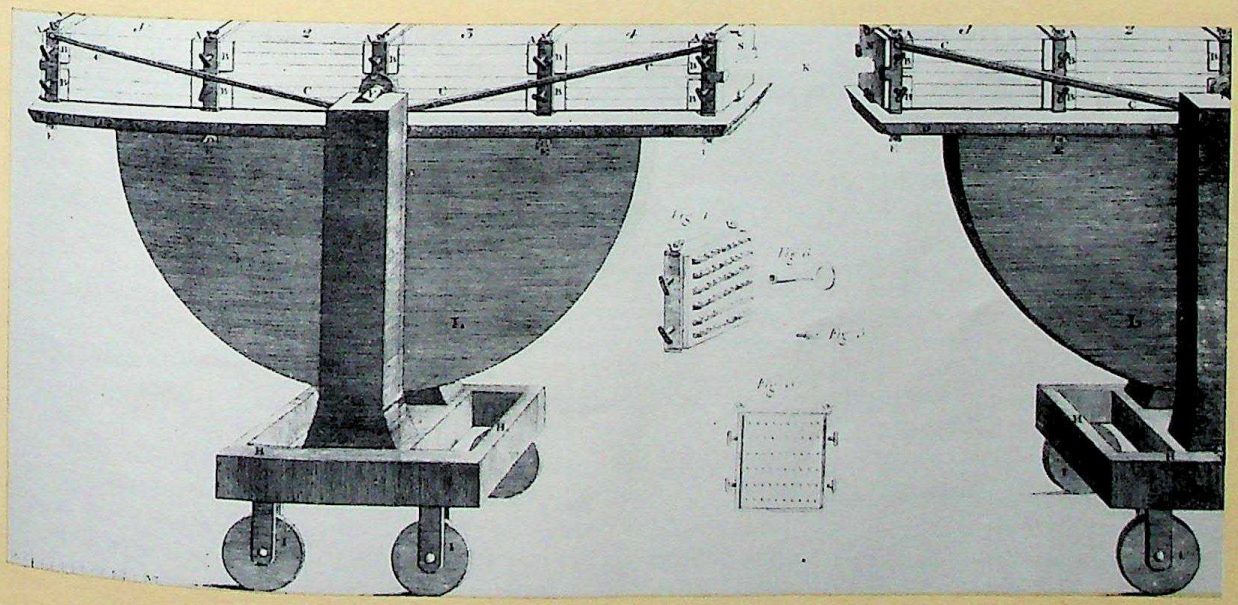

FIGURE 7 – *Fothergill's representation of Knight's magnetic magazine.*

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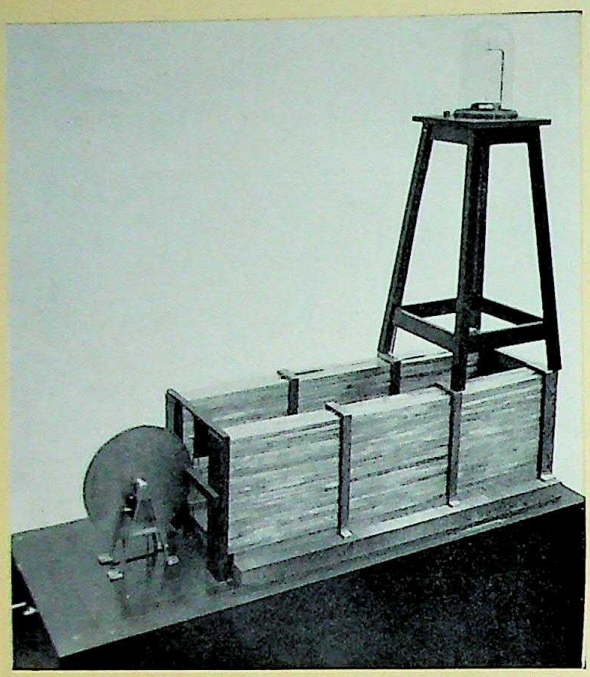

FIGURE 8 – *Adaptation of Knight's magnetic magazine, as used by Faraday.* (Crown copyright, Science Museum, London.)

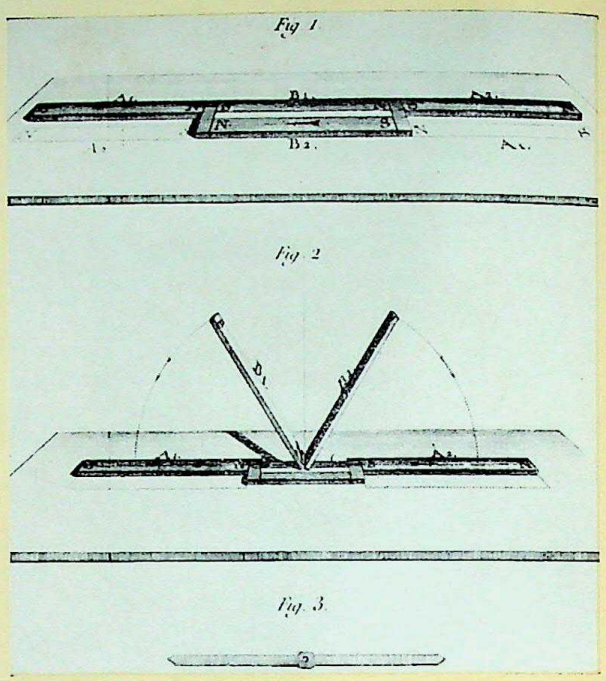

FIGURE 9 – *du Hamel's method of magnetization.*

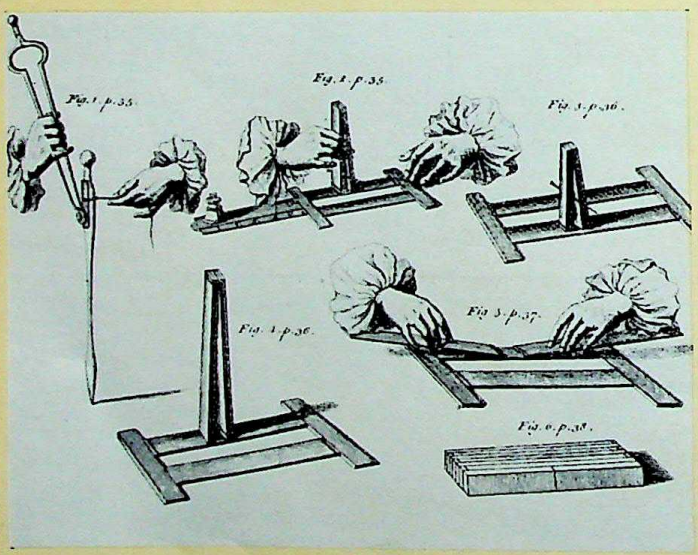

FIGURE 10 – *Canton's methods of magnetizing steel bars.*

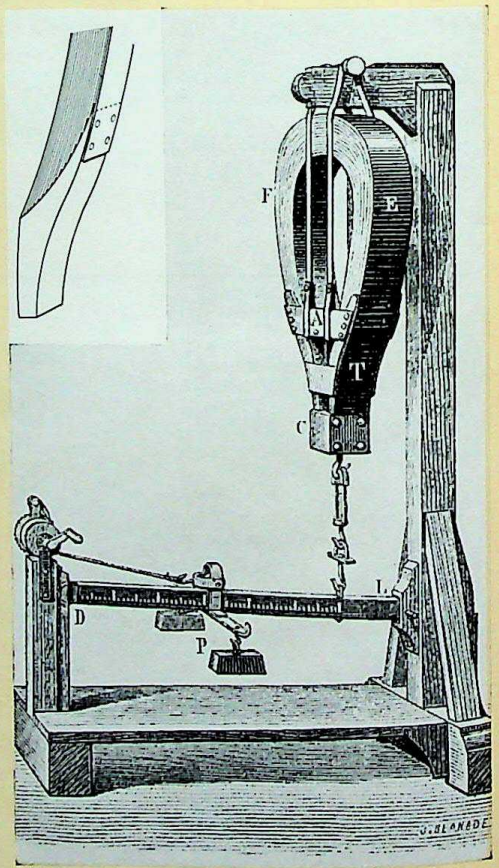

FIGURE 11 (*Right*) – *Jamin's great permanent magnet. The method of fitting the soft iron pole pieces is shown in the inset.*

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poles to like, to which he fitted soft iron endpieces or armatures. This was the first compound magnet, made up of smaller ones. With two of these he magnetized steel wires by the method, invented by him, of divided touch, placing the north pole of one magnet and the south pole of the second magnet close together at the middle of the rod to be magnetized and drawing them apart. He also adopted a method, later used by others, of placing several of the stout wires to be magnetized end to end in a groove and treating them as one, rejecting the end ones. A single one of his wires would lift a nail twelve times its own weight. Savery also made, by a complicated method of mutual touches, large bar magnets, 16 × ¾ × ¾ inch, one of which would sustain another when placed end to end. It must be remembered that the lifting power of geometrically similar magnets varies, *ceteris paribus*, approximately as the 2/3rd power of the weight.

Savery was followed by a man who made a European reputation with his magnets, Gowin Knight (1713–72). The spelling of his name gave his French contemporaries much trouble, the extreme version being the Gnith of Sigaud de la Fond: Coulomb calls him Knigth. He carried out a flourishing business—or, to use the language of a politer day, 'procured considerable pecuniary advantages'—with these magnets: Benjamin Wilson [7] speaks of 'two of his best ten guinea bars'. It may be noted that as the first principal librarian at the British Museum his salary was £160 a year.

Knight kept secret many of his methods, including the way in which he magnetized his initial bars. He did, however, communicate a method of divided touch which he used for his compass needles, which were 5 inches long [8]. Two large bar magnets of hardened steel were placed in line, north pole to south, with a small gap between them; the needle was pressed on to the two ends and the magnets were then drawn apart, the process being repeated a few times. George Adams [9] in 1794 described the method illustrated in figure 5, in which the ends of the bar to be magnetized are supported by permanent magnets, and the method of divided touch, with inclined magnets, is employed. The inclined magnets are placed together, north pole to south, as shown, and then drawn apart, the process being repeated a few times and applied to each surface of the bar C. He says that this was one of the methods used by Knight. This George Adams was the son of George Adams the elder, likewise an instrument maker, who was closely associated with Knight in the making of compasses, so that this assertion is likely to be correct. The method is substantially that later used by Coulomb [11].

Knight was particularly renowned for his 'magnetic magazine', a very powerful compound magnet built up of steel bar magnets each 15 × 1 × ½ inch. J. Fothergill [10], who described the magazine after Knight's death, says that these individual magnets were 'impregnated by repeated attritions' but nothing more of the method. Each half of the compound magnet consisted of 240 bars, in four lengths end to end, each being 6 magnets deep and 10 wide (figure 7), so that the approximate overall dimensions were 5 feet 4 inches long by 6 inches by 5 inches. This magnet was presented to the Royal Society by Fothergill, who had inherited it, in 1776. One half had not long before been impaired by fire and replaced by inferior magnets. Later Christie arranged the two halves to form a horseshoe-pattern magnet, which Faraday used for his famous experiment with the rotating copper disk in 1831. Figure 8 is a photograph of the actual magnet as arranged for this experiment, now exhibited in the Science Museum.

As examples of the strength of Knight's magnets, which so impressed his contemporaries, a bar weighing 53 grams lifted 28 times its own weight of iron, one weighing 435 grams lifted 12 times its own weight, while a magnet compounded of 12 bars, weighing in all 620 grams, lifted 14 times its own weight.

In 1938 a distinguished authority, writing in the *Journal of the Institution of Electrical Engineers*, referred to an 'entirely novel' type of permanent magnet prepared in Japan from powdered oxides by Kato and Takei. However, such magnets were made by Gowin Knight in a manner that he kept secret, but which was published in 1779, after his death, by Benjamin Wilson [7]. He put a large quantity of iron filings in a tub of clean water, which he worked backwards and forwards for some hours until he had a suspension of very finely divided iron oxide. He poured off the water and let the particles settle to a sludge. When, by repeating the process, he had sufficient fine oxide powder he mixed it to a stiff paste with linseed oil, which he moulded to desired shapes and baked before a moderate fire. These blocks he then magnetized between the poles of his magnetic magazine and apparently produced very strong magnets in this way. There was formerly one in the British Museum, but I have been unable to trace its fate.

Contemporary with Gowin Knight were John Canton (1718–72) and John Michell (1724–93).

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Canton [12] used as his original magnets pokers and tongs that had been heated and left to cool upright. 'The larger they are, and the longer they have been used, the better.' He magnetized small bars of iron by holding them, one by one, to the poker and stroking with the tongs, as shown in figure 10. He then placed two large steel bars parallel to each other, yoked with two shorter soft iron bars, and to effect magnetization stood upright on the steel his small magnets, in pairs, north pole touching south at the top, and slightly separated at the bottom by a large pin. He thus produced an intense field over a short distance in the steel. He then rubbed the grouped magnets up and down the bar. This is the method of double touch. With the magnets so produced he magnetized larger bars by double touch. Finally, he used these magnets to magnetize other hard steel bars by divided touch, as also shown in figure 10: in his words 'the horizontal touch, after the vertical, will make the bars as strong as they can be.' His bar magnets weighing $1\frac{3}{4}$ ounce would lift sixteen times their own weight of iron.

In the same year (1750) Michell published his 'Treatise on Artificial Magnets', the first book devoted to the subject of making steel magnets. He made his first magnets with a comparatively short loadstone resting with both poles on a steel bar and rubbed to and fro (a variety of double touch), and then used these magnets, three and three, for vertical double touch as described by Canton. Apparently the two had worked out the method independently. He also supported the touching magnets vertically in a wooden frame, yoking them at the top by soft iron (figure 12).

In the middle of the eighteenth century England was the acknowledged home of the permanent magnet, and the highest claim of those in other lands was that their magnets were as good as Knight's. Important work, was, of course, carried out elsewhere, notably in France and Russia. Aepinus [13] used the method of double touch with inclined magnets mounted in a wooden frame which he slid backwards and forwards like a carpenter's plane. du Hamel (also spelt Duhamel) [14] formed a rectangle, as did Canton, but added two stationary magnets, as shown in figure 9, while he used the method of divided touch. Coulomb says that Canton used the method of du Hamel, but although both papers appeared in the same year, 1750, Canton would appear to have priority, for his was read before the Royal Society on 17th January and du Hamel's is dated 8th April. Antheaulme [15] arranged two

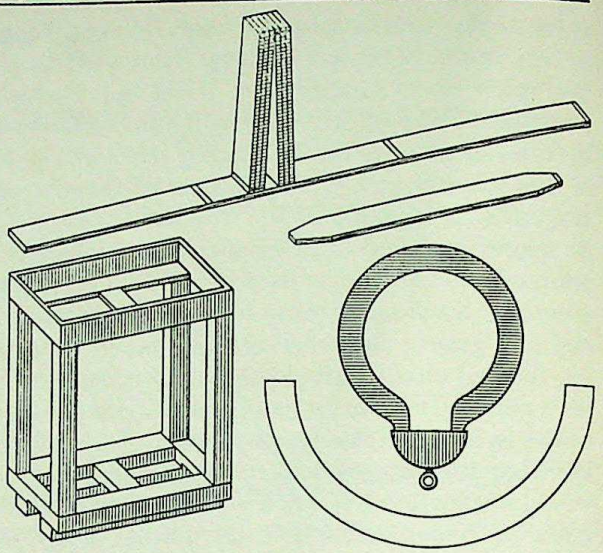

FIGURE 12 – *Michell's illustration of the method of double touch.*

iron bars, each some 15 feet long, in line with a small gap between them, and so inclined to the vertical as to lie in the direction of the Earth's field. At the end of each bar adjacent to the gap was a small projection, or 'heel' (*talon*): the bar to be magnetized was rubbed slowly on these heels. Good results were claimed.

There were many minor differences of methods of touch used by experts in the eighteenth century, but on the whole it is evident, from the figures given, that the best magnets of the period were surprisingly good.

An interesting point about the so-called magnetization by touch, which should be called by rubbing, is the part played by friction. Friction is apparently essential: it is recorded (e.g. by Robison [16]), that oiling the travelling magnets very much impairs the magnetization communicated, as does any other effective lubricant, e.g. talc or gold leaf. Haldat [17] says that steel wires placed in a field so weak as to produce spontaneously but little magnetism acquire marked permanent magnetization if rubbed with any hard body, such as brass, glass, or hard wood. He concludes that any vibration is favourable for magnetization.

Two examples have been given of compound magnets, made up of a number of similar magnets side by side, Servington Savery's small hexagonal bundles and Knight's great magnetic magazine. Thick bars cannot be magnetized all through to saturation by any of the methods of touch, in spite of the instructions given by many of the early

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magnetizers to turn the bar over and touch two opposite, or all four, sides. Compound magnets have the advantage that the comparatively thin rods of which they are composed can be magnetized to saturation. However, the individual magnets when placed together with like poles in contact tend to demagnetize one another. Coulomb [18] carefully investigated this matter with assemblies of thin permanent magnets roughly 6 inches long and 1 inch broad and found, for example, that twelve side by side gave a 79 per cent loss as compared to the sum for the same magnets separate. Scoresby [19] in 1832 carried out similar experiments, which led him to make powerful permanent magnets of steel strips separated by air and joined at the ends by soft iron pole-pieces, as represented in figure 13. Such magnets are frequently represented in nineteenth-century textbooks. Nobili, about the same time, improved the compound magnet by diminishing the length of the strips to either side of the central one (figure 14).

With the development of the electromagnet and the decline of the wealthy amateur who, in the eighteenth century, was a customer for scientific curiosities, the study of the permanent magnet was less warmly pursued. There was no industrial demand. However, round about 1873, J. C. Jamin, known to all physicists for his work on optical interference, devoted much attention to the subject [20] and made what he claimed to be the most powerful magnet in the world: it weighed 50 kg and supported a load of 500 kg. His magnets were made of thin sheets of steel—'*les lames minces s'aimantent à cœur.*' In his large horseshoe magnet such magnetized sheets were successively sprung into a horseshoe-shaped sheet of steel screwed to a soft iron armature, as shown in figure 11. The way the sheets, of varying length, were adapted to the armature is shown in the inset. Jamin chose

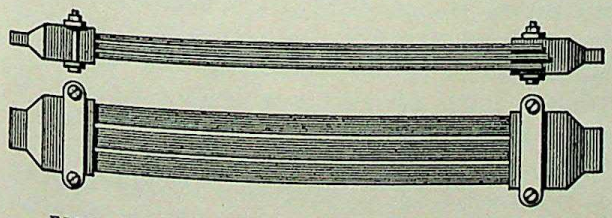

FIGURE 13 – *Scoresby type of permanent magnet.*

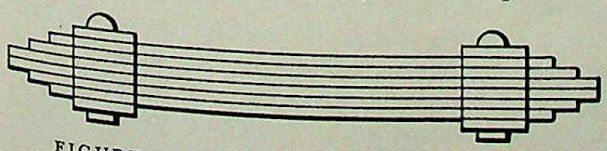

FIGURE 14 – *Nobili type of permanent magnet.*

his steel with great care and went systematically into such questions as the relative volume of the pole-pieces. He also made bar magnets of sheets separated by air gaps, as Scoresby had done.

Early work on alloys is of interest in view of the great recent development, for permanent magnets, of magnetic alloys, such as Alnico, containing considerable proportions of metals which, when unalloyed, have feeble magnetic properties. In 1758 Arderon [21] asserted that in certain circumstances brass could be magnetic and decided that the effect was not due to traces of iron. In particular, he found that non-magnetic brass could be converted into magnetic brass by hammering and subsequent double touch. He suggested prophetically: 'Would some ingenious man pursue these experiments perhaps we might have needles made of brass to act as strongly as steel ones.' Similarly Tiberius Cavallo [22], in 1786, recorded that brass could be made magnetic by hammering and the magnetism destroyed by heat, and concluded from experiments in which the 'texture' was altered that 'the property of becoming magnetic in brass by hammering, is rather owing to some particular configuration of its parts, than to the admixture of any iron.' There are many records of brass compass boxes being magnetic, the effect being generally attributed, without any proof, e.g. by Michell, to iron in the zinc.

Sturgeon [23], who made the first electromagnet in 1825, carried out extensive investigations of the magnetic properties of alloys. Having found specimens of silver coinage then in circulation to be magnetic, he pursued experiments which led him to the conclusion that various alloys of copper—with silver, with gold, and with zinc —were magnetic. Careful analysis convinced him that the effect was not due to iron. He showed, for instance, that 1 part of copper to 5 of silver gave a highly magnetic alloy, while 1 part of iron to 20 of antimony gave an alloy practically non-magnetic, as was zinc with a high percentage of iron. This work, published in 1846, showed that the magnetic properties of alloys were not simply determined by the properties of the constituents. Although fundamental for the modern permanent magnet, it seems to have attracted little attention. As regards brass, Sturgeon had been inclined at first to doubt Cavallo's results, but finally states: 'Indeed I am now inclined to embrace the opinion of Cavallo, that the magnetism of brass is not due to the presence of ferruginous material, but depends upon a suitable arrangement of the particles of its proper constituent metals, copper or zinc.'

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By 1867 it had been recorded in a German handbook that magnetic alloys could be made of non-magnetic materials and non-magnetic alloys of magnetic materials, particularly iron. The development of magnetic alloys, particularly the study of the effect of adding non-magnetic metals to iron and to nickel, belongs to the present century and so is outside the scope of this brief account. Mention may, however, perhaps be made of the Heusler alloys, since they are over fifty years old, the first publication being in 1901. They are non-ferrous ferromagnetics, and began with a non-magnetic manganese-copper alloy, the addition of aluminium to which produced a striking ferromagnetism. A typical Heusler alloy has the composition Cu_2MnAl, but in general the range of composition is 10–30 per cent manganese and 9–15 per cent aluminium, the remainder being copper. Many of the modern permanent magnet alloys, for instance the various Alnicos and Alcomaxes, contain substantial percentages of aluminium and copper, but contain iron, nickel, and cobalt as well. Their properties and development are outstanding examples of the successful application of accumulated knowledge which the methods of modern metallurgy and applied physics have produced. In contrast, the story which is here attempted is, perhaps, remarkable for the results attained with very simple means when metallurgy as a science was in its infancy and applied physics was represented by a few men of great scientific gifts following their personal predilections.

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The hydration of ions in solution

R. P. BELL

Aqueous solutions of ionized substances are of exceptional importance and there is a vast literature describing and discussing their properties. It has long been assumed that in such solutions ions may be closely linked with one or more molecules of water, but measurements by different methods of the degree of hydration long gave very conflicting results. More recently, however—and with a clearer picture in mind of the structure of water itself—much more consistent values have been obtained, making possible some useful generalizations.

Liquids are less perfectly understood than either of the other two states of aggregation, because they possess neither the well-defined geometrical arrangement of solids nor the freedom from intermolecular action of gases. This generalization applies even to liquids composed of essentially spherical atoms, such as those of mercury and the rare gases, and even more to common solvents which consist of less symmetrical molecules. It might therefore be thought that liquid mixtures and solutions would present a still more intractable problem, but in fact certain properties of solutions—the so-called 'colligative' properties, which depend upon the number of molecules and not their nature—can be treated without any assumption of knowledge of the interactions between solute and solvent. The laws of dilute solutions are well known: though solutions of electrolytes do not obey these laws exactly, even at high dilutions, they can be modified to take into account the electrostatic forces between the ions. The theory of interionic attraction is rightly regarded as one of the great successes of physical chemistry: in fact, its success sometimes obscures our ignorance of other fields of solution chemistry.

Only rarely can we ignore solute-solvent interactions, which are of primary importance in determining many of the individual properties of solutions, such as density and viscosity. These interactions are maximal when the solute is ionic, and the simplest picture of such solutions involves solvation, i.e. the definite attachment of solvent molecules to the ion. Most of our knowledge of solvation relates to aqueous solutions. This is perhaps unfortunate from a theoretical point of view, because, as we shall see, the nature of liquid water itself is still not fully understood. However, the importance of the problem is obvious, since ionic hydration influences all equilibria and transport problems involving ions, and thus has a bearing on a wide variety of industrial, laboratory, and biological processes.

From the earliest days of the ionic theory it has been suggested that in aqueous solutions ions are hydrated, the idea probably first arising from observation of the frequent occurrence of water of crystallization in solid salts. That water molecules should become attached to an ion is reasonable from a molecular point of view. Since the charge distribution in a water molecule can be represented as $^{\delta+}H\!-\!O^{\delta-}\!-\!H^{\delta+}$, cationic and anionic hydration can be represented by $X^+ \ldots O(H)(H)$ and $Y^- \ldots H\!-\!O\!-\!H$ respectively.[1]

Unfortunately the concept of hydration was often used with more enthusiasm than discrimination, and little attempt was made to define clearly what it implied. In particular, hydration was frequently invoked simply as a convenient means of accounting for discrepancies between observed properties and those predicted by some theory of solution of doubtful validity, rather as 'association' was used to explain discrepancies in the behaviour of solutions of non-electrolytes. This led to the kind of situation described in 1949 by J. O'M. Bockris [1], who pointed out that different methods and authors had produced the following figures for the hydration number of the sodium ion: 1, 3, 4, 17, 71, and (approximately) 700. It is not surprising that there was much dissension on the subject [2], and that the whole concept of hydration in solution fell into disrepute.

[1] Hydration of cations is sometimes considered to involve a co-ordinate link, but it seems probable that an electrostatic formulation is closer to the truth: in any case the distinction between the two pictures is not a sharp one.

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In the period 1920–40 the great successes of the interionic attraction theory in accounting for the properties of very dilute solutions of electrolytes produced a considerable prejudice in favour of 'physical' rather than 'chemical' theories of solution, solvation being placed in the latter class. However, it has become increasingly evident that consideration of the immediate surroundings of ions in terms of electrostatic theory is not really profitable. For example, Born's well-known expression [3] gives the electrostatic energy of an ion in a medium of dielectric constant D as e^2/Dr (where e = charge and r = ionic radius). If this is to be applied logically, however, it is quite incorrect to use the ordinary macroscopic value of D, since the solvent in the immediate neighbourhood of the ion is subject to enormous electrostatic fields and hydrostatic pressures, which will greatly modify the effective value of D. No exact theory of these effects is available, and the 'effective dielectric constant' thus approaches the status of an indefinitely adjustable parameter. Under these conditions 'physical' treatment has no advantage over 'chemical' treatment as a means of representing the interaction between the ion and the solvent. This difficulty does not arise when considering the variation with concentration of properties of dilute solutions, as in the successful applications of the interionic theory. It does, however, affect any attempt to account for the properties of different salts or of solutions in different solvents, and also any treatment of concentrated solutions.

During the last ten or fifteen years there has been a renewed interest in the solvation of ions, especially in aqueous solutions. This has been partly because of the difficulties of the electrostatic treatment, outlined above, but also because modern methods of investigation have provided a clearer understanding of the nature of liquid water, and of the way in which it can be modified by the presence of ions. In particular, it is recognized that although an ion can have some effect on water molecules at a relatively large distance, molecules in its immediate neighbourhood will be bound particularly firmly: consequently it is often legitimate to distinguish between primary and secondary hydration. The concept of a primary hydration number is useful only to the extent that a single number will account for a variety of properties of a given ion. All the molecules concerned will contribute to the effect of hydration on different properties, but the contribution of each will depend upon the particular property in question. In the following paragraphs some account will be given of the main properties affected by hydration, with special emphasis on primary hydration. Some estimated hydration numbers are given at the end of the article.

MEASUREMENT OF HYDRATION OF IONS

One of the oldest methods of estimating ionic hydration is based upon the transport of water that occurs when a current passes through a solution. A classical method of detecting this transportation [4] is to add to the solution a non-electrolyte, which is assumed not to be transported by the current. The change in the concentration of this reference substance then gives a measure of the net transport, i.e. the difference between the amounts of water transported by the anions and the cations. Unfortunately there is now a good deal of evidence [5] that non-electrolytes of the type usually employed are also transported with the ions: in fact, it seems likely that any suitable non-electrolyte will owe its solubility to the presence of polar groups, and will therefore become attached to ions in the same way as water itself. Even if this difficulty could be overcome, the method still has the inherent disadvantage that it gives only the difference between the hydration numbers of cation and anion and not absolute values.

A somewhat similar, but more satisfactory, approach involves the interpretation of diffusion coefficients of electrolytes in concentrated solutions. This method is of recent date, and depends upon great advances both in the theory of diffusion and in experimental methods. The detailed theory is complicated [6], but it is easy to see qualitatively that the transport of water will affect the observed diffusion coefficient in concentrated solutions but not in dilute ones, so that information about the water transport can be obtained from accurate measurements over a large concentration range. Since both ions move in the same direction this method gives values for the sum of the two hydration numbers, and certain assumptions are needed to obtain the individual values. It seems likely that only the first layer of water molecules will be bound firmly enough to move as a unit with the ion, so that primary hydration numbers should be obtained.

Many attempts have been made to interpret ionic mobilities in terms of solvation, and methods entailing measurement of these have the great advantage that we obtain quantities referring to individual cations and anions, rather than to the

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sums or differences. It was pointed out as long ago as 1915 [7] that the mobilities of the alkali cations ($Li^+ = 38{\cdot}7$, $Na^+ = 50{\cdot}1$, $K^+ = 73{\cdot}5$, $Rb^+ = 77{\cdot}8$) increase as the ionic radius increases, which is the opposite of what one would expect. This can clearly be accounted for qualitatively by assuming that the hydration of the cation decreases along the series, but it is difficult to interpret this quantitatively in terms of a definite hydration number. There is considerable doubt about the absolute hydration numbers derived by this method, but they have reasonable values and show the same trends as those derived by other methods.

The methods so far described relate to various transport phenomena, but there are thermodynamic methods based on the effect of hydration upon the equilibrium properties of ionic solutions. These have the advantage that the underlying theory is better understood than that of transport phenomena: on the other hand, the latter may give information about the interactions in the immediate neighbourhood of an ion, whereas study of any thermodynamic quantity necessarily gives an overall measure of the mutual interaction of all parts of the system. The most direct thermodynamic method derives from the salting-out of non-electrolytes by added electrolytes. If water is attached to the ions there will be less free water to dissolve the non-electrolyte, so that the extent of hydration can be calculated from the observed solubility decrease. This type of argument has been extensively used, but unfortunately the results obtained vary according to the non-electrolyte chosen, and there seems no doubt that this simple picture of salting-out is misleading [8].

A more fruitful approach concerns the properties of the electrolyte solution itself. The use of colligative properties, especially freezing point depressions, is a very old one, many of the early estimates of hydration of solutes being based on observed deviations from Raoult's Law. In a concentrated solution the hydration of solute particles can appreciably decrease the molar fraction of free water, thus leading to such deviations. However, the earlier treatments of this problem neglected all other reasons for departure from ideal behaviour, in particular the ionic forces. In order to avoid the absurdity of large negative hydration numbers for dilute solutions it was necessary to introduce spurious 'degrees of dissociation', usually derived from conductivity measurements, so that little meaning can be attached to these early values. N. Bjerrum [9] was the first to relate interionic forces and hydration. The same idea has appeared again recently in the work of R. H. Stokes and R. A. Robinson [10], and of E. Glueckauf [11] in their interpretations of the activity coefficients of electrolytes (mostly derived from measurements of freezing points or vapour pressures) over a large range of concentrations.

Both treatments allow for the effect of interionic attraction by the standard expression first derived by Debye and Hückel. All deviations from this expression are attributed to the removal of free water by hydration of the ions, the detailed interpretation of these deviations differing in the two treatments. It is found possible to reproduce the observed activity coefficients by assuming for each ion a fixed hydration number that is independent of concentration, and (in Glueckauf's treatment) independent of the ion of opposite charge with which it is associated. The main criticism of this treatment is that the hydration term must really depend upon a number of other factors, such as ion-pair formation and variation of dielectric constant. However, there seems no doubt that hydration is of major importance in concentrated solutions. It has similarly been shown recently [12] that the rapid rise of the acidity function of strong acids with concentration can be mainly attributed to the hydration of the hydrogen ion beyond the stage H_3O^+.

Since water molecules attached to an ion will have only a limited freedom of movement, it is natural to examine ionic entropies in solution as a source of information about hydration. Experimental data lead directly to values for the entropies of dissolved salts, but it is difficult to split these values up into the separate contributions for anions and cations: a number of different methods have been devised, leading to somewhat different final values [13]. Fortunately the uncertainties thus introduced are not great, and in any case do not affect the relative entropy values for a series of anions or cations. These relative values show trends which can reasonably be interpreted in terms of hydration. For example, the entropy becomes more negative with increasing ionic charge, and also with decreasing radius, as illustrated by the following sequences of solution entropies,

$Na^+ +14$, $Mg^{2+} -32$, $Al^{3+} -76$;

$Mg^{2+} -32$, $Ca^{2+} -11$, $Sr^{2+} -7$, $Ba^{2+} +2$;

$F^- -2$, $Cl^- +13$, $Br^- +20$, $I^- +25$.

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However, great difficulties arise when we attempt to interpret these figures quantitatively in terms of ionic hydration. This is most strikingly shown by the fact that the solution of two gram-atoms of argon actually involves an appreciably greater loss of entropy (31 e.u.) than the solution of gaseous K^+ and Cl^- to give aqueous potassium chloride (23 e.u.). Thus while both solution processes lead to a state of greater order, the result of replacing two uncharged argon atoms by the ions K^+ and Cl^- is to decrease the ordering effect, which is just the opposite of what would be predicted on the hydration theory.[1]

There are many similar anomalies, and the problem is far from being solved, even qualitatively. There is no doubt, however, that the root of the problem lies in the associated structure of water itself, which we shall now consider briefly.

THE STRUCTURE OF WATER IN RELATION TO IONIC HYDRATION

If we compare the physical properties of water with those of closely related hydrides, e.g. NH_3, H_2S, HCl, we find that its boiling point, heat of vaporization, entropy of vaporization, and dielectric constant are all unexpectedly high. These, and many other anomalies, can be explained qualitatively by the assumption that liquid water does not consist of single H_2O molecules, but is highly associated. This has been realized for a long time, and the older picture described water as a mixture of molecules H_2O, $(H_2O)_2$, $(H_2O)_4$, etc., the proportions of which were affected by temperature and by the addition of salts. Although this idea has been recently revived by A. Eucken and his collaborators [14], the current picture of association is a rather different one. J. D. Bernal and R. H. Fowler [15] pointed out in 1933 that the X-ray diffraction patterns given by liquid water closely resemble those of ice, which is known to have an open tetrahedral structure held together by hydrogen bonds, and they suggested that the liquid has a similar quasi-crystalline structure. Later work has suggested that the structure of the liquid either involves smaller units or is more flexible than originally supposed [16], but there is no doubt that some degree of order is present, and will be affected by solute molecules.

The presence of many uncharged solute molecules helps to stabilize the open structure, thus decreasing the entropy. For an ion we must imagine an inner zone, in which the water molecules are strongly orientated by the charge, surrounded by a region of disorder in which the incompatible ordering effects of the ion and of the tetrahedral water structure largely annul one another. This complex problem has been considered in detail by many authors, notably H. S. Frank and M. W. Evans [17], but it cannot be said that entropy data can yet be related simply to primary hydration, which is only one of the factors involved. The same is true of the effect of ions on the specific heat of water, which, like the entropy, is too sensitive a property to be related simply to a hydration number.

A more useful line of approach is through consideration of the effect of ions on the volume of water, i.e. from the apparent volumes of ions in solution. These apparent volumes are always less than those in the crystal, and may even be negative for multiply charged ions: this is clearly because the normal open structure of water is replaced by a more closely packed arrangement of water molecules round the ion. However, it is difficult to estimate the volume decrease thus produced, and to derive absolute hydration numbers from it.

The compressibility of water is also decreased by the presence of ions, and the simplest interpretation of the facts is that the water in the primary hydration shell is already compressed to the maximum extent, the remainder having its normal compressibility. On this assumption hydration numbers are readily calculated, and some of the results are given in the table at the end of this article. The use of this method has been stimulated by the development of ultrasonic methods for measuring compressibilities of liquids [18].

CONCLUSION

There are many other methods for studying the hydration of ions, and the literature is vast. It will be realized that the concept of a 'hydration number' is not a very well defined one, and that different methods will give different kinds of average values. Nevertheless, if we confine ourselves to properties expected to depend mainly on primary hydration, there is some measure of agreement between different methods, as is shown by the following table. Different authors have given somewhat different values, and the figures in the table have for present purposes been rounded off to the nearest whole number.

[1] The numerical values of the entropies of solution depend on the standard states chosen. The figures given here correspond to concentrations of one mole per litre both in the gas phase and in solution.

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Estimated hydration numbers

Ion	Method			
	Mobilities	Diffusion	Activities	Compressibilities
Li^+	5	3	3	4
Na^+	4	1	2	5
K^+	4	1	1	5
Mg^{2+}	12	—	5	12
Ca^{2+}	10	—	4	10
La^{3+}	13	—	8	18[1]
F^-	—	—	2	4
Cl^-	4	0	1	2
Br^-	2	1	1	1
I^-	1	1	1	1

[1] Value for Pr^{3+}.

The agreement between the numbers in this table is far from perfect, but it constitutes a great improvement on the kind of figures quoted at the beginning of this article, and the actual values are reasonable. Examination of these values, and many more, leads to the following generalizations:

(i) the hydration number of cations increases with increasing charge and decreasing size,

(ii) with the exception of the fluoride ion, anions are less hydrated than cations.

There are a number of modern techniques which can give special information about the state of water in the neighbourhood of dissolved ions, though so far they have been applied in isolated cases. Thus isotope exchange using ^{18}O has shown that the water molecules attached to chromic ions in solution exchange only slowly with the rest of the solvent, and the method fixed the hydration number of this ion as 6. With other ions investigated the exchange was too fast to measure, but measurements on exchange equilibria gave some information about hydration [19]. Similarly, proton magnetic resonance gives information about the electrical environment of the protons in the water molecule, and about the way in which this is changed by the presence of dissolved ions [20]. Finally, the fine structure of the absorption spectra of the ions of transition and lanthanide elements depends upon the geometrical symmetry of their surroundings: this has so far been investigated mostly in crystals, but in a few instances it has been used to draw conclusions about the arrangement of water molecules round an ion in solution [21]. The last two methods obviously hold great promise for future investigation.

To sum up, it may be said that the concept of ionic hydration in solution is necessary in order to account for the individual properties of dissolved ions, and also for the behaviour of concentrated electrolyte solutions in general. The idea of a fixed primary hydration number is a useful first approximation in dealing with a number of properties, but a complete understanding of the problem must take into account the peculiar associated structure of water, and the effect of ions upon it even outside the primary hydration sphere.

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Le Palais de la Découverte

The *Palais de la Découverte* in Paris, a unique institution for the exposition of science, this year celebrates its twentieth anniversary. Like a number of other French museums, it owes its origin to an international exhibition, in this instance the International Exhibition of 1937. The *Palais* was set up as an adjunct to the exhibition as a means of showing visitors, in a spectacular yet strictly accurate way, some of the fundamental discoveries of modern science. Its success exceeded all expectations, for within the space of six months there were more than two million visitors: at times the crowds were so great that the doors had to be closed. As a result it was decided to make the *Palais de la Découverte* permanent, and it reopened in June 1938. Originally placed under the Minister of Education, it is a part of the University of Paris, and is administered by a council of twenty, of which the Rector is president. It has since become not only an important feature of the scientific life of France but has attained an international reputation. It has staged many special exhibitions of material lent from abroad—such as the penicillin exhibition of 1946 and the Chagas exhibition of 1955—and has also on many occasions lent its own material to the principal scientific museums of the world.

The great importance to science of general understanding of its aims and methods needs no emphasis. There are many means of seeking to achieve this aim—journals, books, radio, and television, to mention but a few of the most important—but among them museums and exhibitions occupy a special place, for they alone give the visitor an opportunity of actually seeing, and perhaps even of handling and using, scientific material and apparatus. Descriptions and pictures can be very informative but cannot have the same reality as the object itself. The fifty-three galleries of the *Palais* contain a wealth of striking exhibits representative not only of the principal branches of science and mathematics but of medicine and surgery. Models and demonstrations are well supported by descriptions, diagrams, and photographs, and to a limited extent films: colour is freely used. There is throughout a most skilful blending of artistry and science, and this is doubtless in large measure a reflection of the interests of the Director. Auxiliary exhibits are used to clarify particular points. A special feature is the number of trained attendants available to answer visitors' questions.

In 1952 a planetarium was added, and this is one of the finest in Europe. The importance of such equipment for stimulating wide interest in astronomical problems—and thence in science generally—is very great, although all too little appreciated. In this connection it may perhaps be remarked in passing that the first planetarium in the United Kingdom is now nearing completion and will be opened in London later this year.

The exceptional interest aroused by the planetarium is well sustained by the astronomical section. One entire room is devoted to the stellar universe. Enormous photographs and many models illustrate the principal features of the universe, and a special section is devoted to the sun. The moon, too, is accorded special attention, and recent events have of course greatly stimulated interest in this satellite. There is a great lunar globe, built on a scale of 1 mm per km, a remarkable reconstruction of a lunar landscape, and a model which demonstrates how eclipses occur.

The *Palais* caters for visitors of three different kinds. Firstly, members of the general public who are interested in scientific matters. Secondly, organized cultural groups. Thirdly, schoolchildren taken round by their teachers. This last group is a large and enthusiastic one, and it is not unusual for schools to make visits ten or fifteen times a year. For them, the experimental demonstrations —for example, of the wave theory of light—are of great importance, for many of these are ones which are beyond the resources of the ordinary teaching laboratory. Some four hundred experiments are conducted daily, and a further two hundred can be arranged at a week's notice.

In brief, the *Palais de la Découverte* is a generously conceived machine for the popularization of science, a point of union between the laboratory and the public, and an institution for scientific teaching at a high level. It is above all a living museum—representative of fields of science which are of current importance and still developing—rather than a collection of exhibits of things which have had their day and are now of only historical interest. There is, nevertheless, a small section devoted to the history of science: this usefully emphasizes the continuity of the subject and helps to set the modern exhibits in perspective. After twenty years, the first disrupted by war, the *Palais* has achieved much and is seeking to achieve still more. Its organization is not static but seeks to keep pace with the tremendously rapid advancement of science. It is a task in which all will wish it success.

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Water repellency

N. K. ADAM

Water will collect in droplets on ordinary smooth surfaces if the contact angle exceeds 100°, but the largest contact angles recorded for smooth surfaces do not greatly exceed 105°. As almost all surfaces slowly soak up water, so diminishing the contact angle, their water-repelling properties are not permanent. Roughening of the surfaces, however, may increase the contact angle, and values as great as 160° have been recorded for certain surfaces containing regularly spaced air gaps. This effect has important practical consequences.

The power of altering any surface to make it water-repellent is of great practical value in many ways. Tents and raincoats are obvious examples of the necessity of surface waterproofing. The vast mineral flotation industry provides another kind of example. In the froth-flotation process small quantities of 'collectors' are added to a slurry of ground ore in water; this results in the formation of a water-repellent film on the surface of the particles of the valuable constituents of the ore, so that these attach themselves to small air bubbles blown into the slurry and rise with the bubbles to the surface. A rather less well known instance is 'dropwise condensation'. If, in a condenser, the cooled surface exposed to the steam is clean, condensation occurs as a continuous film, which offers much more resistance to the passage of heat than would the metal surface alone. The efficiency of a condenser can be increased if a water-repellent film can be maintained on the condensing surface, so that the steam condenses as discontinuous drops, leaving most of the surface free from the water film. Water repellency is also of great biological importance: thus the survival of many plants and animals depends upon their ability to shed water from their surfaces.

For smooth surfaces the theory of water repellency is well known. If the angle of contact between the liquid-air surface and the solid is θ, and γ_S, γ_L, and γ_{SL} are the surface tensions of the solid, liquid, and the solid–liquid interface:

$$\gamma_S = \gamma_{SL} + \gamma_L \cos\theta \qquad \ldots.(1)$$

Dupré's equation relates the work of adhesion W_{SL}, i.e. the work required per unit area to separate the liquid from the solid, to the surface tensions:

$$W_{SL} = \gamma_S + \gamma_L - \gamma_{SL} \qquad \ldots.(2)$$

From (1) and (2) it follows that

$$W_{SL} = \gamma_L(1 + \cos\theta) \qquad \ldots.(3)$$

This may be called Young's equation, since it was given, in words and without proof, in Thomas Young's 'Essay on the Cohesion of Fluids', published in 1805 [1]. This equation shows that the contact angle θ is determined by the relative magnitudes of the adhesion of the liquid for the solid, W_{SL}, and the self-cohesion of the liquid, $2\gamma_L$. For a given value of the surface tension of the liquid, the contact angle increases as the adhesion between liquid and solid decreases. An angle of 180° would indicate zero adhesion, but if there is contact between a liquid and a solid there is always some adhesion, so that angles as large as 180° are never found in practice. Equation (3), it may be remarked, is far more useful than (1), since the latter contains the two solid surface tensions γ_S and γ_{SL}, which cannot be measured with any accuracy.

Water repellency is greatest when the contact angle is as large as possible. If it is over 100°, the liquid assumes the form of separate droplets which run over the surface fairly easily and are not difficult to shake off. The largest contact angles so far recorded with water on smooth surfaces are between 105° and 110°; such angles are obtained when the outermost layer of the surface consists almost entirely of methyl groups. Paraffin wax shows this angle. Polyethylene [2] gives a rather smaller angle (94°). The difference between the two substances is usually ascribed to the surface of the wax consisting of CH_3— groups, while the surface of polyethylene, effectively a paraffin of enormously high molecular weight, probably consists of little but methylene, —CH_2—, groups. The largest recorded contact angle between water and a smooth surface is 110°; this was for a carefully purified crystal of hexatriacontane [2]. This angle indicates the adhesion to water to be 48 ergs per square centimetre, almost exactly the same as that between a liquid paraffin and water.

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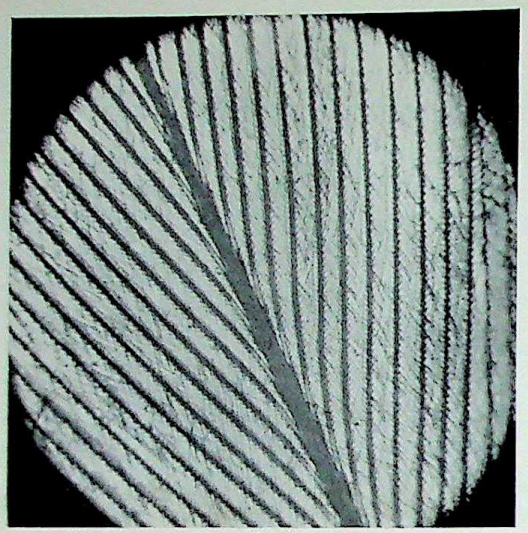


FIGURE 1 – *Duck's feather.*

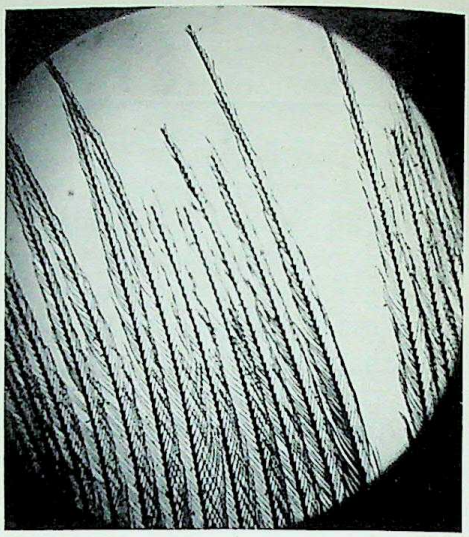


FIGURE 2 – *Duck's feather, teased out.*

FIGURE 3 – Salvinia *leaf, from above.*

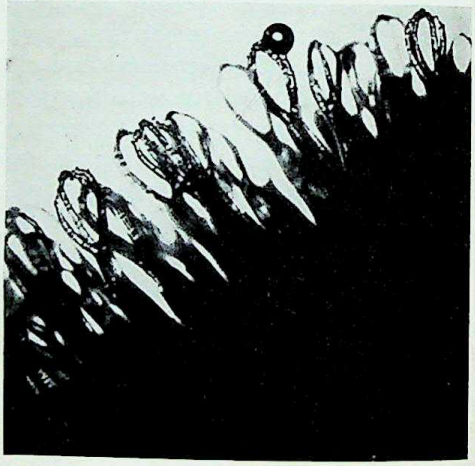
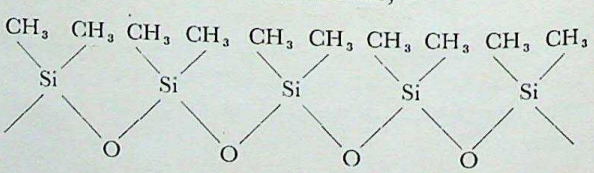


FIGURE 4 – *Projections on* Salvinia *leaf, in profile.*

Surfaces of solid long-chain fatty acids show an angle usually between 100° and 105°. But if a solid mass of stearic acid is cut with a knife [3], the angle may be anywhere between 50° and 105°. This is because crystalline stearic acid consists of plane leaflets two molecules thick, with the water-attracting carboxyl groups sandwiched together between the long hydrocarbon chains. If the knife has travelled parallel to these leaflets, only hydrocarbon groups are exposed, since the adhesion between two hydrocarbon groups is very much smaller than that between two carboxyl groups. If, however, the knife has travelled across the leaflets, some carboxyl groups are exposed at the surface, which thus attracts water more strongly and gives a lower contact angle than does the pure hydrocarbon surface.

The attraction for water of polar groups buried below the surface extends for only a very short distance. Normally hydrophilic or water-attracting surfaces can be made almost as water-repellent as paraffin wax if they are covered with a film only one molecule thick of a long-chain fatty acid [4], or amine [5], or a heavy-metal soap. Treatment of a surface with a silicone,



which deposits an exterior layer in which only a single layer of methyl groups intervenes between water and the —Si—O—Si—O—Si—O—Si—

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structure, raises the contact angle to such an extent that water collects in small drops instead of forming a film over the surface.

Replacing the hydrogen atoms by fluorine appears, from the rather few published observations of contact angle, to decrease the attraction for water. Polytetrafluorethylene (P.T.F.E. or Teflon) gave an angle of 108°, 14° higher than for polyethylene [6], and a monolayer of perfluorodecanoic acid, $C_9F_{19}COOH$, on copper [7] gave an angle of 102°. It seems probable that on a perfluorinated paraffin wax, water would show an angle slightly greater than 110°.

The highest contact angle obtainable with any natural smooth surface is therefore not likely to be much over 105°. This would be sufficient to provide quite good water repellency if it could be relied upon not to decrease on prolonged contact with water or exposure to a high concentration of water vapour. Unfortunately water soaks to some extent into almost all solids, slightly even into paraffin wax, so that the contact angle on a continually wetted surface decreases more or less rapidly. This decrease in angle after contact with water is one, though probably not the only one, of the causes of the so-called 'hysteresis' of contact angles. The 'advancing' angle obtained when the liquid is advancing over a dry surface is larger, sometimes by a considerable amount, than the 'receding' angle obtained when the liquid recedes from a previously wetted surface. For permanent water repellency the receding angle must stay large, preferably not below 90°, otherwise there is a tendency for the water to remain behind as a more or less continuous film instead of 'pearling' off the surface as mobile drops. It is very noticeable with many rainproofed fabrics that at the beginning of a shower raindrops can easily be shaken off; but later the whole surface becomes wet, and eventually rain may start penetrating right through the pores of the cloth. On a smooth surface an initial contact angle even of 105–110° does not give a sufficient factor of safety to achieve permanent water repellency.

Some plants and animals overcome this difficulty by developing surfaces with broken-up structures. It was noticed in the 1920s that roughness increases the contact angle if the material is of such a nature that it would give an angle greater than 90° if the surface were smooth. If, however, the angle on the smooth surface is less than 90°, roughness of the surface decreases the angle. Simply roughening a surface exaggerates the amount by which the natural contact angle differs from 90°. This effect was given quantitative form by R. N. Wenzel [8]; if r is the roughness factor, i.e. the ratio of the real to the apparent surface, the average contact angle $\bar{\theta}$ on the rough surface is given by

$$\cos \bar{\theta} = r \cos \theta \qquad \ldots . (4)$$

where θ is the angle on a similar but smooth surface.

In 1944 A. B. D. Cassie and S. Baxter [9] worked out the theory of average or apparent contact angles on surfaces where there are regularly spaced air gaps, as in the feathers of birds. They explained for the first time how the proverbial water-shedding property of the duck's back is due to the combination of a wax layer having a contact angle not much over 90°, with a special structure which the duck maintains by careful preening.

If for each square centimetre of the apparent surface the liquid actually touches solid over a total area f_1 cm², and the air gaps in the surface total f_2 cm², the average contact angle $\bar{\theta}$ is given by

$$\cos \bar{\theta} = f_1 \cos \theta - f_2 \qquad \ldots . (5)$$

θ is here the 'real' angle on a smooth surface with the same attraction for water as the solid parts of the composite surface. Equation (5) holds for both advancing and receding angles.

The fractions f_1 and f_2 were calculated for a set of parallel cylinders of diameter $2r$, regularly spaced with centres $2(r + d)$ apart; these are shown in section in figure 5. Figure 6 shows the average advancing angles $\bar{\theta}_A$ plotted against $(r + d)/r$ for various values of θ_A. Similar, but not identical, curves were obtained for the average receding angles $\bar{\theta}_R$. In both cases the average

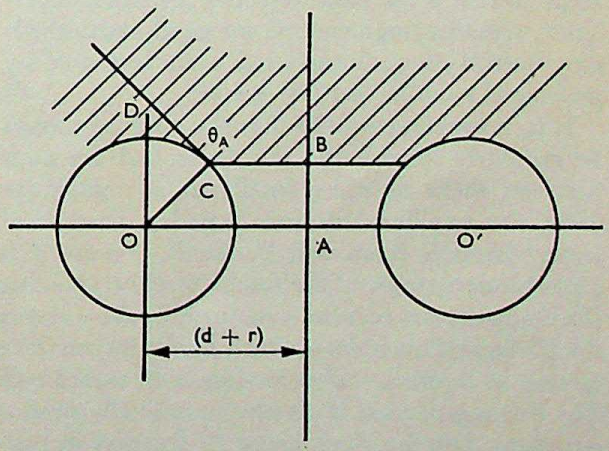

FIGURE 5 – *Calculation of f_1 and f_2.*

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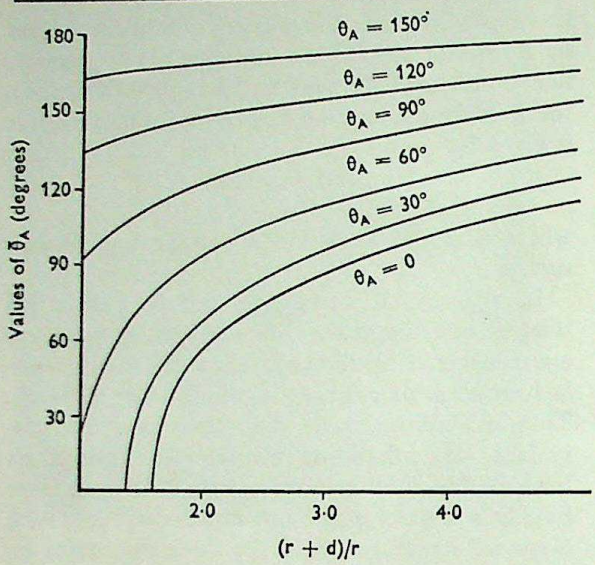

FIGURE 6 – *Values of average contact angle* $\bar{\theta}_A$ *for various values of* θ_A *and* $(r + d)/r$.

angle increases rapidly as the gaps between the fibres grow larger. For a set of parallel wires with diameter 0·07 mm, coated with a paraffin wax which on a smooth surface gave a 'real' advancing contact angle $\theta_A = 105°$ and a receding angle $\theta_R = 93°$, the average advancing angle $\bar{\theta}_A$ was 145° and the average receding angle $\bar{\theta}_R$ was 138°. The width of the gaps was in this instance about 2½ times the diameter of the wires. The measured angles were very close to the calculated. A striking feature of the curves in figure 6 is that the average angles may be greater than 90° even when the real angles are far less. Thus the air gaps not only increase the initial contact angle, when the water first touches the surface, to an extent which makes it far easier for the drops to run off, but provide a 'factor of safety' whereby the average contact angle, which is the angle effective for water repellency, remains large enough for good water-shedding even if the real angle has been decreased by prolonged contact with the water.

It is, however, essential that the air gaps should be regularly spaced and the fibres and the gaps between them must be small. If any gaps are wide, the capillary pressure resisting passage of water through them will be small. Figure 1 is a photomicrograph of the duck's feather, showing the compact but regular structure; figure 2 shows the barbs and barbules—which normally interlace as seen in figure 1—after having been teased out. The feathers of most birds have a basically similar structure, but the duck keeps its feathers in particularly good condition. The domestic hen's feathers often have the gaps blocked with a continuous film, which impairs the water repellency. The barbules of the breast feather of the duck are about 8 μ in diameter, and the ratio $(r + d)/r$ is roughly 5. The barbules carry hooks and notches which interlock and prevent the barbules being drawn together by the surface tension of water adhering to the surface.

The contrast between fur and feathers is very marked: in fur the fibres are not held at their ends and therefore gather into compact tufts when wet. The spaces between the fibres are thus eliminated and the water repellency is reduced to that of a continuous surface.

The aquatic plant *Salvinia* sheds water perhaps even more effectively than the duck. Figures 3 and 4 show that the surface of the leaves is furnished with projections whose outer end is a loop of hydrophobic material. Water in contact with the leaf touches solid over only a small part of its area, so that in equation (5) f_1 is small and f_2 large. The result is that even after prolonged immersion the *Salvinia* leaves appear practically dry on removal from the water.

Some aquatic insects utilize a similar principle to enable them to remain under water almost indefinitely and to make use of dissolved oxygen for respiratory purposes. Much of their body surface is covered with very fine hydrophobic hairs which are nearly perpendicular to the surface for most of their length, but bend over at their tips so as to be nearly parallel to the surface. This layer of hairs entraps a practically constant volume of air, so that a thin layer of air is in permanent contact both with the insect surface and with the water. The hairs and the layer of air constitute the 'plastron', which functions as a kind of physical gill although there is no continuous membrane separating the water from the air. The structure

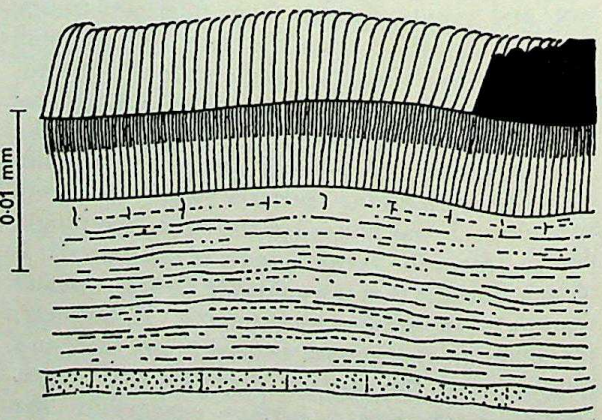

FIGURE 7 – *Plastron of* Aphelocheirus.

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and functioning of the plastron have been very thoroughly studied by W. H. Thorpe and D. J. Crisp [10]. Figure 7 shows, diagrammatically, the structure of the plastron of the water-bug *Aphelocheirus aestivalis*. The hairs are about 0·18 μ diameter and the gaps between the hairs about 0·4 μ, corresponding to over two million hairs per square millimetre.

A pressure of at least two atmospheres is required to force water through the very fine mat formed by the tips of the hairs, and even if the surface tension and contact angle are substantially decreased by the presence of some surface-active impurity in the water, water has still great difficulty in penetrating into the air space. The rigidity of the hairs prevents appreciable decrease in the volume of the air in the plastron. Respiration occurs by absorption of oxygen from the plastron, this being replaced by diffusion of oxygen from the water outside. The plastron enables the insects to live almost permanently under water, and in this respect is much more efficient than the air bubbles which some insects attach to their bodies so as to provide a supply of air. Such air bubbles are not protected against changes in pressure, and they decrease in volume as the oxygen passes out of them into the insect and the nitrogen into the surrounding water. Insects that rely on attached bubbles for their supply of air under water have at intervals to come to the surface or to seek fresh bubbles under water.

The arrangement and shape of the minute hairs in *Aphelocheirus* is ideal for retaining a thin layer of air whose volume does not change. If the hairs were perpendicular to the surface over their whole length, they would entrap air only so long as the contact angle of the water with the hairs remained greater than 90°.

These considerations suggest possible ways of improving the waterproofing of textiles. Improvements are likely to depend on two factors. Firstly, the finding of substances which absorb water less than the best present agents, so that the contact angle will for a longer time remain large enough for drops to pearl off, instead of forming a continuous film. Secondly, devising a structure in which the position of threads and air gaps is maintained with such stability that the threads cannot draw together and obliterate some gaps, leaving others larger than when the fabric is dry.

ACKNOWLEDGMENT

Figures 5 and 6 are reproduced from the 'Transactions of the Faraday Society' and figure 7 from the 'Journal of Experimental Biology,' by kind permission of the Editors.

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Radioactive tracers and fertilization in mammals

R. G. EDWARDS and J. L. SIRLIN

The importance of understanding the development of the spermatozoon and the ovum, and of the embryo resulting from their fusion, needs no emphasis. The experimental difficulties are, however, considerable. The use of radioactive tracers makes it possible to follow labelled spermatozoa within the egg and to study the interactions between nucleus and cytoplasm.

Knowledge of the processes of fertilization and early embryonic development in mammals has considerably increased in recent years. New techniques, such as the examination of living eggs and embryos under the phase-contrast microscope [1] and the incorporation of radioactive tracers into eggs and ovaries, have improved upon previous methods. Our intention here is to show how the use of tracers in studies on gametogenesis (i.e. the production of eggs and spermatozoa), fertilization, and early embryonic development is contributing to knowledge of the physiology of reproduction in mammals. These studies relate mainly to the mouse.

The principal technique used to detect and locate tracers incorporated into tissues has been autoradiography. From autoradiographs (figures 1–3), the localization of tracer inside a cell can be resolved to within about 2 μ. This permits the identification of labelled cells as compared with unlabelled cells in the same tissue. Certain tracers are incorporated into the developing sex cells in the seminiferous epithelium of the testis or in the ovary, and are retained in the cells during their maturation into gametes; it is therefore possible to follow the progress of spermatogenesis in the testis, or of oogenesis in the ovary. The labelled spermatozoa or eggs that result can then be used in studying fertilization and early embryonic development. The resolution of the autoradiographs is also sufficient to study in detail the intracellular distribution of tracer, and thus the technique has become an important tool in cell biochemistry.

SPERMATOGENESIS

Two days after an intraperitoneal injection of phosphorus-32 as phosphate into male mice, the tracer in the testis was incorporated mainly into spermatogonia (the first stage of development into spermatozoa) and remained in the cells during their development through the successive stages of spermatogenesis [2]. More recently other tracers, methionine-^{35}S [3] or adenine-8-^{14}C [4, 5], have been used in preference to phosphorus. Adenine also was incorporated mainly into spermatogonia soon after the injection [5]. It may also be taken up by spermatocytes (the next stage in spermatogenesis), for this tracer is incorporated into deoxyribonucleic acid (DNA), and the last synthesis of DNA during spermatogenesis occurs in young spermatocytes [6]. Methionine was incorporated into both spermatogonia and spermatocytes [3]. These tracers, especially adenine, were retained in the cells throughout spermatogenesis [2–5], and thus the progress of spermatogenesis could be timed by means of autoradiographs of the testes of males killed at various intervals after injection of the tracer.

The estimates obtained by this method were: 5 days from the most mature spermatogonia to the first meiotic prophase (spermatocytes) [5]; 8 days to early spermatids [5]; 18–25 days to spermatozoa free in the testis and epididymis [3–5]; and 30 days to ejaculated spermatozoa [4, 7]. The last figure was obtained by, at regular intervals, mating males injected with adenine and making autoradiographs of the ejaculated spermatozoa. The autoradiographs were negative, i.e. no tracer was detectable in the spermatozoa, until approximately 30 days after the injection of adenine. Labelled spermatozoa, i.e. spermatozoa showing a positive autoradiograph, as shown in figure 1, occurred in increasing numbers from 30 days after adenine was injected up to a maximum at 38 days, thereafter decreasing steadily [4, 7]. This increase is probably due to an increasing number of spermatogonia being labelled for some days after the injection [5, 7].

Spermatozoa can be labelled with formate-^{14}C, but this is much less effective than adenine [7].

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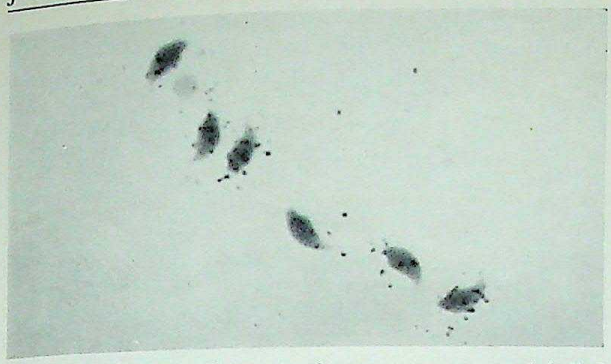

FIGURE 1 – *Autoradiograph of mouse spermatozoa labelled with adenine-8-^{14}C. The photographic grains can be seen in the emulsion overlaying the sperm-heads; not all grains are in focus.* (× 900)

Glycine and methionine did not label individual spermatozoa to any significant extent [7]. The difference between adenine and the other tracers must arise from the fact that in mature spermatozoa adenine is incorporated mainly into DNA, which is relatively abundant and remains metabolically stable after meiosis. Some adenine may also have been incorporated into the mid-piece of the spermatozoon, though in very small amounts as compared with the sperm-head [7].

The tracer method is therefore of value in estimating the intervals between spermatogonia and the later stages of spermatogenesis. Because the tracers are incorporated into all three types of spermatogonia (A, intermediate, and B) they are not of direct use in timing the development of the different types. However, other means of estimating these intervals are available. The different types of cells in the seminiferous epithelium do not occur at random but in characteristic associations, and the relative duration of each association of cell types has been estimated by determining their frequencies [8]. The absolute durations have been estimated by X-irradiating males and studying the course of the subsequent progressive damage in the seminiferous epithelium. Spermatogonia B are relatively the most easily destroyed by X-rays, and this results in the progressive disappearance from the seminiferous epithelium of the subsequent stages of spermatogenesis [9, 10]. By killing males at various intervals after irradiation and then finding the cell associations from which particular spermatogenic stages have been eliminated, the time taken for each type of spermatogonia to mature, and also for later stages of spermatogenesis, was estimated. A disadvantage is that X-irradiation may retard spermatogenesis, though probably not excessively.

By the X-irradiation method, the interval between the development of spermatogonia A and the appearance of spermatozoa free in the testis, has been estimated as 34·5 days in the mouse [10], and the intervals between the stages of spermatogenesis have also been measured. There is reasonable agreement between the estimates obtained by this method and those obtained with tracers [7]. By combining the results of both methods, the total duration of the spermatogenic cycle—from spermatogonia A to ejaculation—has been estimated as 42 days [7].

The measurement of the relative durations of cell associations in the seminiferous epithelium of the ram [11] has been combined with tracer technique [12]. This combination reduces the possibility of a delay in spermatogenesis due to irradiation, particularly when tracers are used that emit soft radiation. The prophase of the first meiosis in the ram was calculated to take 15 days, spermiogenesis 14 days, and the total duration of spermatogenesis was 49–51 days [12]. After injecting phosphorus-32 into cocks, the radioactivity of seminal plasma was a maximum after one week [13], and of spermatozoa after two [13] or more [14] weeks.

THE USE OF LABELLED SPERMATOZOA

Labelled spermatozoa assist study of the fate of spermatozoa in the female reproductive tract, for by the use of autoradiography their presence can be detected even though they may have lost their typical shape and become unrecognizable under the microscope. In mouse eggs fertilized by labelled spermatozoa, only one of the two pronuclei was labelled [15]. The labelling must have been of the male pronucleus, and by this identification the results of previous investigations by other workers into the morphology of the male and female pronuclei were confirmed. Some tracer which was found in the cytoplasm of these eggs has provisionally been ascribed to the path of sperm entry into the egg [15]. Male chromosomes could be identified by their autoradiograph in the prophase of syngamy [7]. Labelled spermatozoa were also seen between the cumulus cells that surround the eggs [15].

Some naturally occurring or experimentally treated eggs do not possess two pronuclei after fertilization. In one egg which contained three pronuclei, two of them were labelled and the egg was therefore dispermic [15]. This illustrates the value of labelled spermatozoa in the identification of pronuclei, though the number of male and

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female pronuclei can generally be more easily determined by counting the number of sperm-tails in a living egg under the phase-contrast microscope. In eggs possessing only one pronucleus, however, identification of the pronucleus is best achieved by using labelled spermatozoa, for it is difficult to gain this information from living eggs.

If some of the many excess spermatozoa which fail to fertilize the eggs of the female do nevertheless fulfil a useful function—e.g. if they penetrate into and become part of the uterus, the fallopian tubes, or other tissues ('somatic fertilization')—they should be detected in autoradiographs of these tissues. No positive autoradiographs of these tissues have so far been detected in mice, however [7], and this supports other evidence against the reality of somatic fertilization [16]. Many of the excess spermatozoa are removed from the uterus by leucocytes; in the uterine contents of females previously mated to injected males some labelled spermatozoa were found ingested by leucocytes [7]. Other leucocytes were labelled though no spermatozoa could be seen inside them, the ingested spermatozoa having apparently been destroyed.

Spermatozoa form only a small part of the ejaculate, the bulk of it being seminal plasma. By injecting other tracers into males (glycine-2-^{14}C, methionine-^{35}S), the seminal plasma was labelled without appreciable labelling of the spermatozoa [17]. Labelled seminal plasma was ejaculated for only a few days after injection of the tracers, and its activity had decreased considerably 10 days after injection [17]. Labelled seminal plasma could be used in experiments similar to those described above for labelled spermatozoa, and the distribution in the female tract of the two components of the ejaculate could then be compared. No convincing evidence has so far been found of tracer in the eggs of females mated to males ejaculating labelled seminal plasma: either the seminal plasma did not enter the eggs at all or it entered in amounts too small to be unequivocally identified by autoradiographs [17].

MATURATION OF THE EGG

Detailed studies using tracers have been made of the physiology of the ovary and the oocyte. The granulosa cells of rabbit, rat, and mouse will incorporate phosphorus-32 [17–20], glycine-2-^{14}C [17], adenine-8-^{14}C [17], sulphate-^{35}S [21–24], or methionine-^{35}S [24] in considerable amounts (figure 2), as a result of the high mitotic rate [20, 25] and secretory function of this tissue. Atretic follicles

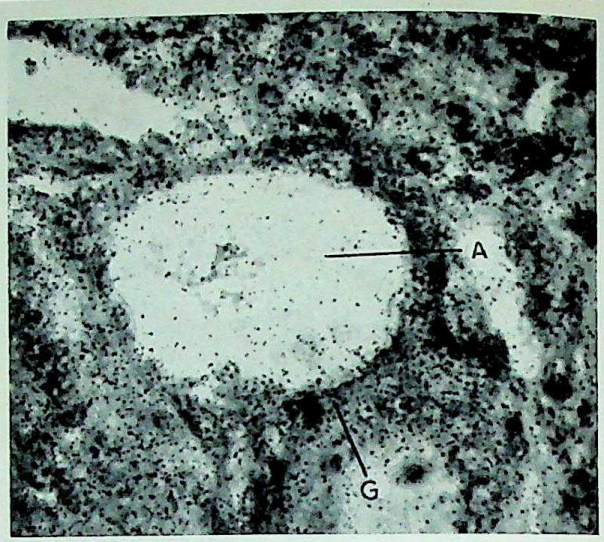

FIGURE 2 – *Autoradiograph of a sectioned mouse ovary labelled with glycine-2-^{14}C. The granulosa (G) and, to a less extent, a young oocyte (A) are labelled. (Reproduced by permission of the publishers of the Proceedings of the Second World Congress on Fertility and Sterility, Naples, 1956.)* (× 900)

incorporate less tracer than normal follicles [18, 20, 22]. Autoradiographs reveal the position of tracer atoms of phosphorus and labelled glycine that are incorporated into proteins and nucleic acids; of adenine incorporated into nucleic acids; of methionine incorporated into protein; and of sulphate incorporated mainly into mucosaccharides. The incorporated ^{32}P has been found to be present in either DNA or ribonucleic acid (RNA) [17, 20]. Caution must be exercised in interpreting data derived solely from phosphorus-32, for

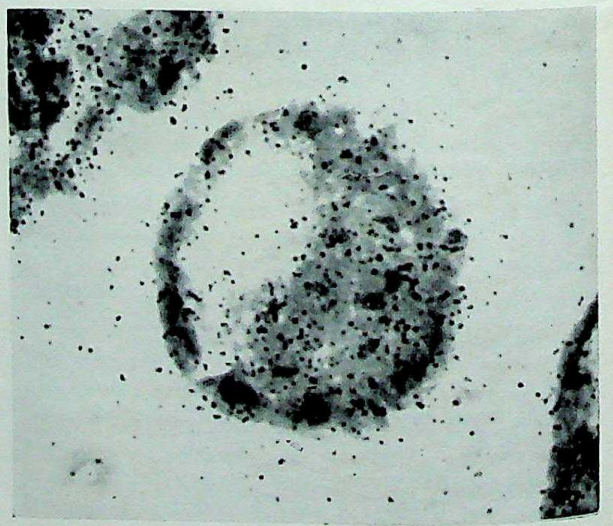

FIGURE 3 – *Autoradiograph of a sectioned mouse blastocyst labelled with glycine-2-^{14}C.* (× 1300)

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the doses necessary to obtain autoradiographs are bound to damage tissue.

The incorporation of phosphorus, adenine, and glycine into the granulosa was very high 36 hours after the injection of tracer into females, but declined rapidly in a few days [17]. Phosphorus and glycine, but not adenine, labelled the follicular fluid to a certain extent; from this difference, and from the rate of loss of tracer from the fluid, it was concluded that the fluid is in equilibrium with the serum [17; see 26]. Sulphate-^{35}S is secreted into the follicular fluid in considerable amounts, probably as a sulpho-mucopolysaccharide [27], after its accumulation in the granulosa [22–24]. The amount of incorporated sulphur-35 depended on the physiological activity of the ovary: the amount in the follicular fluid was greatly increased if the ovarian follicles were stimulated by the advent of puberty or by injections of gonadotrophins, and this response may be of value in assaying follicle-stimulating substances [28]. At ovulation, ^{35}S left the rabbit follicle with the follicular fluid, being probably contained in mucins that assist in ovulation and the transport of eggs down the fallopian tubes [27]. Sulphate was not accumulated into luteal cells of the rabbit [29] or rat [22], though the growing connective tissue of mature and old corpora lutea in the rabbit incorporated moderate and high amounts respectively [29].

Incorporation of tracer was always less into the oocyte than into the granulosa (figure 2), but was still sufficient to allow study of the detailed distribution of tracer in the various cell constituents. From three hours after the injection of phosphorus, glycine, or adenine into mice, small concentrations of tracer were present in the cytoplasm; more was in the nucleus and nuclear wall; and large amounts were in the nucleolus and its wall [17, 19]. These cytochemical observations accord with current ideas on the sites of synthesis of proteins and nucleic acids in the cell. After longer intervals from the injection, more activity was found in the cytoplasm of the oocyte, and this was probably contributed by the granulosa [17]. In the rabbit, phosphorus was concentrated around the periphery of the egg, possibly in the zona pellucida [31], though this was more uncommon in the mouse oocyte [19]. The fallopian tubes of the rabbit secreted large amounts of sulphur-35 (injected as sulphate) associated with mucins, which reinforced the tracer already on the egg membranes [22, 24, 32]. Part of the sulphur-35 in tissue may have been in the form of sulphydryl complexes [32].

In contrast to the labelling of spermatozoa, labelling of mouse eggs was detectable for only a short period after injection of tracers [17]. This difference—probably due to the labelling in the eggs of a more labile chemical system that retained tracer for only a few days—made it impossible to follow the development of particular generations of eggs, as was possible with spermatozoa, and the technique has so far been of no value in timing the stages of oogenesis. Nevertheless, the considerable amount of tracer incorporated into the ovulated eggs before fertilization has been useful in studying fertilization and embryonic development.

DEVELOPMENT OF LABELLED EGGS

Labelled eggs have been used to study some aspects of fertilization and embryonic development. After injection of adenine-8-^{14}C [17], glycine-2-^{14}C [17], phosphorus-32 [17], or methionine-^{35}S [33] into mice twelve hours before ovulation, considerable activity was present in the ovulated eggs. When labelled eggs are fertilized by non-radioactive spermatozoa, all the tracer in the pronuclei must come from the egg. In mouse eggs, the male and female pronuclei each possessed approximately equal amounts of tracer [17], the cytoplasm of the egg having apparently contributed to the growth of the male pronucleus. In these pronucleate eggs, tracer was concentrated in the cytoplasm near the nuclear wall, indicating enhanced synthesis at that site together with, or alternatively, migration of nuclear products; this supports previous observations on autoradiographs of amphibian and chick embryos [30]. Thus a general pattern of the synthesis of protein and nucleic acids appears to hold for various animal classes. In the majority of the pronucleate eggs, approximately one-quarter of the egg had less activity than the remainder [17]; this quarter may correspond to the vacuolated area which, on cytochemical evidence [34], contains no RNA and lies to one side of the polar axis.

Mouse eggs labelled with glycine or methionine have been sectioned at intervals during development from the two-celled stage to blastocysts [17, 33]. In these stages, information was obtained about the intracellular distribution of tracer. The graded distribution of RNA detectable by cytochemical methods [34] in stages between two-celled eggs and morulae could not be identified from the distribution of tracer in eggs labelled with glycine [17], but this gradient might have been obscured by the incorporation of glycine into compounds other than RNA. Labelled eggs and embryos have been transplanted into the uteri of

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females carrying their own embryos of the same age, and into females with no embryos of their own [17, 33], in attempts to identify the transplanted radioactive embryos and to distinguish them from the non-radioactive embryos of the host at later stages of gestation. The labelled embryos were distinguishable in the two-celled stage [33], had sufficient activity remaining for identification as blastocysts at $3\frac{1}{2}$ days gestation [17] (figure 3), but were not identifiable at $7\frac{1}{2}$ days gestation because the tracer had become too diffuse to produce an autoradiograph [17]. The labelling of embryos therefore permits the maternal origins of two types of embryos in the same uterus to be identified up to $3\frac{1}{2}$ days gestation; this is otherwise extremely difficult.

An alternative method of labelling mouse eggs has been attempted [17]. Small amounts of glycine-2-^{14}C were injected through the cervix into the uterus of superovulated mouse females at the time of ovulation, and the females were then mated. The fertilized eggs recovered were found to be labelled; tracer could have entered the eggs either with the spermatozoa or by absorption during passage through the fallopian tubes. Some evidence for the latter was found in that blastocysts were more heavily labelled than pronucleate or two-celled eggs; this would be expected if continued residence in the tubes resulted in the uptake of more tracer. The activity in the embryos was always less than that found after intraperitoneal injections of tracer into females, though smaller amounts of tracer were injected through the cervix [17]. These results indicate the possibility of labelling embryos *in vitro* using small amounts of tracer.

Each developed grain in the autoradiographs represents essentially the radiation from the decay of one radioactive atom. This radiation, originating in the cell itself or in its neighbourhood, could cause genetic changes in each cell of the embryo. Further, after decaying an atom has changed into an atom of a different chemical element, and this new element, produced before or after incorporation into chromosomes, could cause mutations or chromosomal aberrations. After the injection of tracer into females, embryos labelled with sulphate [35], methionine [33], glycine [36], or adenine [36] have developed to birth and maturity, all of them being apparently normal. Offspring bred from these animals to test for induced recessive mutations were also normal, though the experiment was on too small a scale to prove the absence of induced mutations [36]. These tracers emit soft radiations and did not harm the mothers [35, 36], but on the other hand radiation damage of a physiological rather than a genetic nature has been widely recorded from tracers emitting hard radiation, e.g. radio-iodine [37, 38] and radio-phosphorus [36]. Phosphorus-32 in relatively small doses resulted in radiation sickness in the mother and the total absence of offspring, while higher doses suppressed embryonic development to $3\frac{1}{2}$ days gestation [36].

CONCLUSION

We have described how recent work with tracers and autoradiographs is enlarging the scope of studies on the maturation of gametes and the early development of mammals, especially the mouse. Tracer technique applied in studies on the laboratory mammals has been extended to the domestic mammals [11–14], though the price of tracers makes extensive research on these animals somewhat costly. Less expensive adaptations of the technique would therefore be desirable. As an example of a cheaper technique, mouse eggs and embryos have been labelled *in situ* by flushing the uterus and fallopian tubes with solutions containing less tracer than that needed to label ovarian oocytes by intraperitoneal injections [17]. Perhaps it may eventually prove possible effectively to label mammalian spermatozoa [39] and eggs either *in vitro* or by injecting tracers into testes and ovaries.

ACKNOWLEDGMENTS

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Book reviews

EIGENVALUES

Variational Methods for Eigenvalue Problems, *by S. H. Gould. Pp. xiv + 179. University of Toronto Press, Toronto; Oxford University Press, London.* 1957. 48*s. net.*

This work is devoted to an exposition of Rayleigh-Ritz and Weinstein's methods for approximate calculation of eigenvalues. The eigenvalues of elliptic differential equations of mathematical physics may be considered either as possible values for a parameter in the equation itself, or alternatively and equivalently from the variational point of view as maxima and minima of certain expressions. The strength of the variational approach, which forms the subject of Professor Gould's book, lies in the facility with which a given problem can be modified into an approximately similar and soluble problem.

Once a given problem is cast in the variational form, one can do two things: one can introduce more stringent prescribed conditions and find minimum values of the parameter for the new problem. This gives upper limits for the eigenvalues of the original problem and is in essence the Rayleigh-Ritz method. On the other hand, one can weaken the prescribed conditions so that one obtains lower limits for the desired eigenvalues. This is the method of Weinstein. A combination of the two methods will confine the eigenvalue within close limits.

Professor Gould gives a clear and systematic exposition of the concepts involved, developing practically all the mathematics he needs. Notwithstanding the high standard of rigour, he achieves a readability equalled only by a few of the preceding volumes in this series. The book has been a pleasure to read and is warmly recommended to both elementary and advanced students. A. SALAM

HISTORY OF ASTRONOMY

Discovery of the Universe, *by G. de Vaucouleurs. Pp. 328. Faber and Faber Ltd., London.* 1957. 30*s. net.*

It might not be inferred from the title that this book is a short history of astronomy from the earliest times to current developments. To compress such a history into about 300 pages demands careful selection of material, and in this the author has achieved a large measure of success. The book has been written with the emphasis on the development of ideas rather than on biographical details.

The first half of the book covers the period up to about the middle of the nineteenth century, during which period the history of astronomy can be adequately dealt with in chronological order, the number of those who contributed significantly to the development of ideas being then comparatively small. From the middle of the nineteenth century onward developments have become increasingly rapid, and it is more convenient to deal in turn with various branches of the subject. The second half of the book is divided almost equally between the period from the middle of the nineteenth century to the second world war, and from 1945 to the present date.

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The author has endeavoured to cover a very wide field in this last period, so that the text becomes almost a catalogue of different investigations and loses something in interest. A useful selected analytic bibliography and both name and subject indexes are given. It is gratifying that the dates of birth and death of astronomers not now living are given, with few exceptions. H. SPENCER JONES

GALACTIC NEBULAE

Galactic Nebulae and Interstellar Matter, *by J. Dufay, translated by A. J. Pomerans. Pp.* 352. *Hutchinson's Scientific and Technical Publications, London.* 1957. 60*s. net.*

M. Dufay's treatise, *Nébuleuses galactiques et matière interstellaire*, has since its publication provided the most complete and authoritative account of diffuse matter in the Universe, in its various forms—atoms, molecules, and solid particles. It was well worth publication in an English edition. As the subject is in a state of rapid development, M. Dufay has, for the purpose of this edition, revised and extended certain sections, thereby providing a complete account of the information available about interstellar matter up to 1955.

This English edition is well printed and produced, and the plates are excellent. It is unfortunately, however, marred by many defects. The proof-reading has been slipshod; there are errors in formulae and in proper names, as well as many misprints—such as monograms for nomograms, convention for convection, occulation for occultation. The punctuation is bad throughout. Many inappropriate adjectives are used, such as weak stars for faint stars, lightest stars for least massive stars, wide open objective for wide-aperture objective, small temperature for low temperature, large emission lines for broad emission lines, proper brightness for intrinsic brightness. Various terms are incorrectly translated: variable clusters instead of cluster variables; Schmidt chamber instead of Schmidt camera. Many sentences are just nonsense, such as 'the curve . . . has the same appearance as a point', 'thousands of invisible blue stars have been counted on the infrared negative'. The meaning in these cases can usually be inferred from the context, but not infrequently a passage is so unintelligible that it can be interpreted only by reference to the original French text. The construction of many sentences is extremely awkward so that the precision of the French text is completely lost.

All who are interested in this subject and who can read French are recommended to use the original French edition in preference to this poor translation. H. SPENCER JONES

GALACTIC NOVAE

The Galactic Novae, *by Cecilia Payne-Gaposchkin. Pp.* x + 336. *North-Holland Publishing Co., Amsterdam.* 1957. 30.50 *guilders net.*

The novae or temporary stars, which flare up suddenly, increasing in brightness many thousands or even millions of times, and then gradually fade again, are objects of special interest. The phenomena that accompany their outburst are of great complexity. Their spectra show rapid changes; in the early stages several spectra of different types and with different Doppler displacements are superposed. The normal nova increases in brightness by about 10 magnitudes, but exceptional novae —termed supernovae—show a much greater increase. The supernovae are divided into two classes, with increases of about 20 and 15 magnitudes respectively; their spectra differ in character from those of normal novae. No clue is available at present for the interpretation of the spectra of Class I supernovae, while hydrogen is the only element definitely identified in the spectra of Class II supernovae. In addition to the true novae there are many stars that show nova-like characteristics, which may be recurrent, but with a smaller rise in brightness.

There has hitherto been no book in English (although there is one in Russian) devoted entirely to this important class of stars. This excellent monograph fills a notable gap in astronomical literature. The number of papers that have been published dealing with individual novae or with various nova problems is extremely large. The author has missed no paper of importance, and each chapter ends with an extensive bibliography. The available information about all observed galactic novae is summarized, separate chapters being devoted to stars for which first-class data are available, to stars for which the data are second-class, and to stars for which the data are more fragmentary. Two chapters are devoted to the various classes of nova-like stars, and one to the supernovae. The concluding chapters are concerned with a comparative study of spectral development, and with evolutionary and theoretical problems. The nova outburst probably belongs to the final stages of a star's career; there have been many speculations about the underlying cause, but nothing is known with certainty. The author states that 'very likely no Type I supernova has appeared in our galaxy since 1604, and we may hope for another in the not distant future.' We may have to await such an occurrence for the interpretation of the complicated spectra of these stars of exceptional interest.

This book will be invaluable for reference purposes. Only a few errors and misprints, and those of a minor nature, have been noticed. The production and printing are excellent.

H. SPENCER JONES

AMERICAN UNIVERSITY PHYSICS

Currents, Fields and Particles, *by F. Bitter. Pp.* 599 + *xiv. The Technology Press of Massachusetts Institute of Technology and John Wiley & Sons Inc., New York; Chapman and Hall Ltd., London.* 1956. 68*s. net.*

It is always of interest to study the teaching methods of another country as indicated by a well-written textbook. Undergraduate courses can readily become very stereotyped, and in Britain a nearly standard pattern prevails, at least during the first two years. It therefore comes as quite a surprise to find that this book devotes scant attention to geometrical optics. As one reads steadily forward, reaching electromagnetic waves by Chapter 8, the impression is gained that the level reached approaches that of the third year of an Honours course. Chapter 9, however, deals with Optics—reflection and refraction, which represent the uppermost level attained in this subject. It is merely that the relative weight and order assigned to different branches are markedly different from those in Britain.

In many respects Professor Bitter's treatment is more in keeping with modern requirements than is the normal undergraduate course in a British university. There has been in Britain too great a tendency to add new matter to the syllabus without correspondingly reducing the burden by adequate rationalization of the original material.

The special features claimed for this book include an early introduction to the concepts of mass particles, dualism, and the elements of atomic theory. It can be fairly said that in these respects

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the treatment is good and could well be followed in the second year of an undergraduate course in a British university —a level that is roughly that of the book as a whole, due allowance being made for certain basic differences.

The book has been thoughtfully planned, is clearly the fruit of long teaching experience, is well illustrated and easy to read; there are numerous well-chosen examples at the end of each chapter. It would be a very useful and stimulating work for British students to possess, but the price is very high.

H. S. W. MASSEY

MODERN NUCLEAR PHYSICS

Elementary Nuclear Theory (*second edition*), *by H. A. Bethe and P. Morrison. Pp. xi + 274. John Wiley & Sons, New York; Chapman and Hall Ltd., London.* 1956. 50*s. net.*

One of the difficulties of the present hectic state of nuclear and high-energy physics is the scarcity of good books on the subject. Potential authors cannot spare time to write a book which may be largely out of date by the time it is published. The first edition of Professor Bethe's book was very welcome, but this new expanded edition in which Professor Morrison has collaborated is of even greater value.

In a remarkably compact fashion the authors have succeeded in covering most aspects of nuclear physics as it stood towards the end of 1955. The clarity with which often quite subtle arguments are expounded will certainly make the book essential for the research student beginning work on nuclear physics, the university teacher, or even specialists in either the experimental or theoretical side of the subject. Any physicist specializing in other fields who wishes to obtain an idea of the present state of development of nuclear physics would also be well advised to read this book.

The new edition, apart from having been generally expanded, includes new material on nuclear reactions and scattering and on the physics of mesons. Particular attention is paid to the discussion of polarization effects in nuclear collisions. This is both appropriate and useful, as in the last few years much attention has been paid to the experimental and theoretical study of these effects, with very substantial rewards as judged from applications to the interpretation of nuclear forces.

The work now includes three major sections: the description theory of nuclei (7 chapters), the quantitative theory of nuclear forces (11 chapters), and complex nuclei and β-decay (3 chapters). A table of nuclear species is given in the appendix, and there is also a table of numerical relations. H. S. W. MASSEY

NUCLEAR MOMENTS

Theories of Nuclear Moments, *by R. J. Blin-Stoyl. Pp. 88. The Clarendon Press. Oxford.* 1957. 8*s.* 6*d. net.*

This book is the first of a new series entitled 'Oxford Library of the Physical Sciences', which promises to be of great value to those engaged in scientific research. This volume sets a high standard.

The magnetic dipole moments of most atomic nuclei have now been measured to an accuracy of 1 per cent or better, and some nuclear quadrupole and octupole moments have also been determined. Dr Blin-Stoyl presents a critical discussion of the theoretical interpretations of these measurements.

It is at present quite impossible to obtain exact wave functions to represent the structure of a nucleus except for the very simplest cases, and so it is necessary to construct simplified models for which calculations can be made. The principles of the various models are outlined very clearly, followed by a detailed discussion of their use in interpreting measured values of nuclear moments.

This monograph represents a valuable stocktaking of the present position in this field for the expert, but the discussions are at the same time clear enough to be most useful to the research worker not directly concerned with these theoretical problems. R. E. RICHARDS

NEUTRON TRANSPORT

Neutron Transport Theory, *by B. Davison. Pp. xx + 450. Oxford University Press, London.* 1957. 75*s. net.*

At first sight the subject of this monograph does not seem to possess the generality of most of the subjects that have so far been treated in the series. The book, however, reveals surprising ramifications and the need for a considerable mathematical apparatus in dealing with the problems of neutron transport. The author, an acknowledged expert in his field, has drawn on both published and unpublished material to make this surely a standard text for many years to come.

The treatment throughout is not difficult, but the profusion of symbols and the fact that ideas are drawn from so many different fields do not make for easy reading. The more general reader would like to dip into the book at intervals and extract those parts that happen to be of value to him; but it must be admitted that the work does not lend itself to this, and in fact the understanding of its parts requires an understanding of the whole. For that reason it is doubtful whether it will have the general appeal that a similar, but less detailed, book might have had. For those whose primary interest is neutron transport theory it would be difficult to imagine a better book.

L. R. B. ELTON

SEMICONDUCTORS

Progress in Semiconductors, *Vol. II, edited by A. F. Gibson. Pp. vii + 280. Heywood & Co. Ltd., London.* 1957. 63*s. net.*

This is the second volume of a welcome series of reviews. Although there is some continuity between this and the previous text, the editors have wisely chosen to deal with other timely subjects in the field. Semiconductor alloys (Ge–Si) and the III–V intermetallic compounds are well covered, and their growing importance in the fundamental study of energy states in crystals is emphasized. Germanium is given the space of two articles, one on the production of single crystals of high purity and the other on the physical properties of germanium containing controlled concentrations of selected impurities. There are valuable treatments of radiation damage in semiconductors and of the problem of carrier lifetimes in such solids. An account of the effects of high-intensity electric fields on semiconductors is complemented by an article on the theories of electroluminescence.

Each review appears to be up to date and complete with adequate references to literature. Only a brief general index of main topics is included at the end of the text.

The editor is to be commended for his choice of both material and authors.

G. F. J. GARLICK

ELECTRICAL CONTACTS

The Physics of Electrical Contacts, *by E. Llewellyn Jones. Pp. x + 219. The Clarendon Press, Oxford.* 1957. 35*s. net.*

For many years the study of the phenomena associated with the working of electrical contacts has been regarded as chiefly the concern of the engineer: only comparatively recently have the fundamental physical aspects of the

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problem been investigated in detail. Professor Llewellyn Jones's book is therefore particularly timely; he has avoided lengthy technological discussions and has presented a concise and lucid treatment of the basic physics of electrical contacts.

After giving a general review of the subject and of the results of earlier workers, Professor Llewellyn Jones selects for special attention the problem of fine transfer in medium-voltage contacts. He discusses the possible mechanisms, and in particular gives a detailed account of the asymmetry resulting from the Thomson effect. The discussion of the properties of metallic bridges at high temperatures provides an interesting illustration of how the fundamental study of a particular phenomenon can in turn lead to the use of that phenomenon as the basis of a new experimental technique.

The book ends with a disappointingly short chapter on the behaviour of electric contacts when closed (as distinct from that during make and break). Several aspects are mentioned briefly, but the chapter as a whole does not reach the high standard of the earlier parts of this book. J. B. P. WILLIAMSON

COVALENT BONDS

Chemistry of the Covalent Bond, *by L. B. Clapp. Pp. xiv + 684. W. H. Freeman & Co., San Francisco; Bailey Bros. and Swinfen Ltd., London.* 1957. $7.50 *net.*

This book is intended for the first-year course at an American university where the freshmen will have previously had one year of chemistry, little mathematics and physics, and no calculus. It deals with covalent substances, almost all being organic. Although best described as an elementary textbook of organic chemistry, it also contains a general introduction and chapters on oxidation and reduction, acids and bases, and the development of the concept of the covalent linkage. Unlike most elementary books, descriptions of laboratory methods are absent. Most chemists will probably consider this wrong, since chemistry, particularly in the fields dealt with, is usually regarded as essentially a practical subject. After reading this book, the student will have no idea of the preparation or handling of the substances described. The approach is entirely one of considering the properties in terms of the bonds. There are sections on strain theory, stereochemistry, resonance and electronic displacements. Probably in order to stimulate the interest of a certain type of student, there are references to technical applications and to proteins, colour, etc. The book provides an interesting, if unusual, introduction to organic chemistry for students.

J. W. LINNETT

LABORATORY SAFETY

Safety in the Chemical Laboratory *(second edition), by H. A. J. Pieters and J. W. Creyghton. Pp. xiv + 305. Butterworths Scientific Publications, London; Academic Press Inc., New York.* 1957. 40*s. net.*

As a work of reference on laboratory safety this book has much that is commendable, but the preface states that the book is 'primarily intended to be the property of every member of the laboratory staff. It should be used on the laboratory bench, not in the library'. The high price of the present edition makes this very improbable.

Far too much detailed attention is given to certain sections, e.g. analysis, while more important items are scarcely mentioned. Seven pages are devoted to protection of the individual, whereas five lines and one figure are allowed for screens. The most important pieces of safety equipment, however, are adequate screens. This must always be the first line of defence, protection of the individual being considered only because the guarding of the apparatus may not be entirely safe. Too much has been covered in one book, and the emphasis is placed on secondary items. The result is that the book is not large enough for a comprehensive manual on safety and too large for a bench safety handbook. L. J. BURRAGE

NON-ELECTROLYTE SOLUTIONS

The Molecular Theory of Solutions, *by I. Prigogine. Pp. xx + 448. North-Holland Publishing Co., Amsterdam.* 1957. 48 *guilders net.*

This book deals with the difficult problem of relating the equilibrium properties of liquid mixtures to the individual properties of the molecules and the forces between them. Considerable advances have been made in this field during the last two decades, partly stimulated by interest in polymer solutions. Recent books have concentrated on a semi-empirical approach to the problem ('Solubility of Non-electrolytes', by Hildebrand and Scott, 1950) or on the lattice model of the liquid state ('Mixtures', by Guggenheim, 1952); the present book places the main emphasis on the cell model, originally used by Lennard-Jones and by Eyring, and in particular on the average potential cell model which has been intensively studied by Professor Prigogine and his school during the last few years. This is not a book for the general reader, but it will be extremely valuable to those with a primary interest in non-electrolyte solutions, since the theory is often more clearly and logically developed than in the author's original papers on the subject. Comparison with experiment is stressed throughout, and the presentation and production are excellent. R. P. BELL

HETEROCYCLIC COMPOUNDS

Heterocyclic Compounds, *Vol. VI, edited by R. C. Elderfield. Pp. vii + 753. John Wiley & Sons Inc., New York; Chapman and Hall Ltd., London.* 1957. £10 *net.*

This substantial volume is the latest addition to this well-known and much-valued series of reference books. The treatment of six-membered heterocyclic compounds containing two heteroatoms follows the pattern of the previous volumes, and the importance of these ring systems is reflected in the size, and unfortunately also in the price, of the volume.

The fourteen chapters deal with the following ring systems: dioxanes (Kremer and Rochen), benzodioxanes (Elderfield), sulphur analogues of dioxanes (Elderfield), pyridazines (Jacobs) cinnolines (Jacobs), phthalazine (Elderfield and Whyte), pyrimidine (Kenner and W. H. Todd), quinazoline (Williamson), pyrazines and piperazines (Pratt), quinoxalines (Pratt), oxazines (Cromwell), benzoxazines (Elderfield, Todd, and Gerber), thiazines (Elderfield and Harris), and phenazines, phenoxazines, and phenothiazines (Pearson). The major chapters are those on the pyrimidines, the pyrazines, the oxazines, and the phenazines.

As in the previous volumes, the treatment of the different groups is almost wholly concerned with their preparation and chemical properties. There is little discussion of physical properties, reaction mechanisms, or stereochemistry. The several thousand references are mainly from the fairly recent literature and greatly enhance the value of the book, as do the thousands of graphic formulae with which it is illustrated.

W. BAKER

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GEOLOGY

Géologie sédimentaire: les séries marines, *by Augustin Lombard.* Pp. 722. *Masson et Cie, Paris.* 1956. *Frs.* 11 000.

Professor Lombard's large book deals with sedimentary rocks in relation to geology as a whole. This is a big undertaking, but the rhythms found in so many sedimentary rock sequences provide a central theme. No other work deals with this facet of geology in such detail, and the book is to be warmly welcomed.

The first of four parts deals with the present-day marine and lake sediments in relation to their environments. The second part begins the consideration of the marine sedimentary rock series. The third part, on sedimentary associations, forms the core of the book, especially a section of eighty pages dealing with the rhythms shown by sedimentary rocks. Use is made of the concept of ideal rhythmic sequences of various kinds, with which the actual, incomplete, and often accidentally confused sequences are compared. Finally, the genesis of the sedimentary rock series in relation to tectonics and depositional environment is considered.

Professor Lombard's book is not an easy one to evaluate. He has presented his material from an unconventional point of view, which in itself is good. The author emphasizes his own views and hypotheses by printing them in italics, while other conceptions of greater general significance are not highlighted. The book will undoubtedly prove stimulating, but its price is high, and many geologists might have preferred a less bulky book at lower cost.

L. R. WAGER

EVOLUTION AND RELIGION

Evolutionary Theory and Christian Belief, *by David Lack. Pp.* 128. *Methuen & Co. Ltd., London.* 1957. 10*s.* 6*d.*

Dr Lack has already written some very attractive studies of bird life. But here he turns to the broader problems of evolution and its possible conflict with Christian belief. As befits one who is Reader in Ornithology at Oxford and Director of the Edward Grey Institute, it is a most carefully documented study. The author writes as one for whom the essential validity of the theory of evolution is now beyond dispute, and yet as one for whom the acceptance of this theory, so far from solving problems, opens new ones. As the sub-title of the book ('The Unresolved Conflict') suggests, Dr Lack is unwilling to fall down on either side of the fence, though he very cogently points out how impossible it must always be to accept the significance of thoughts and judgments made by a human mind if that mind is nothing but the product of some evolutionary process. For once in a way here is an absolutely fair book: from the nature of things it is not a very profound work, and can be read by anyone with just a sprinkling of scientific knowledge. The whole story of the so-called conflict between Darwinism and Christianity has been bedevilled by so much bad spirit, and bad history, that it is refreshing to see both sides of the dispute stated so clearly and reasonably.

C. A. COULSON

PURINES

The Chemistry and Biology of Purines (Ciba Foundation Symposium), *edited by G. E. W. Wolstenholme and Cecilia M. O'Connor. Pp. xii* + 327. *J. and A. Churchill, London.* 1957. 48*s. net.*

For biochemistry this is the era of the study of metabolic and biosynthetic pathways, an inevitable outcome of the application of radioisotopes to biochemical research. The purines have received their full share of attention—one might even say a liberal share—since one of the most promising approaches to the cancer problem appears to be through a detailed understanding of purine synthesis, nucleic acid formation, and the precise relationship of nucleoproteins to protein synthesis. The pteridines are closely involved in some of these transformations, and it is understandable that they also are the subject of much activity.

The volume under review sets on record a Ciba Symposium attended by the leading research workers in the fields of purine and pteridine chemistry and biochemistry. The emphasis is largely chemical, and a debt of gratitude will be owed by many to the sponsors of the symposium and to the publishers, whose volume provides an authoritative and up to date exposition of the work going on and of results achieved in this vitally important field. The high standard of presentation of previous volumes is maintained, the clarity of formulae and figures deserving special praise.

C. RIMINGTON

THYROID HORMONES

Ciba Foundation Colloquia on Endocrinology, *Vol. X,* Regulation and Mode of Action of Thyroid Hormones, *edited by G. E. W. Wolstenholme and Elaine C. P. Millar. Pp. xii* + 311. *J. and A. Churchill Ltd., London.* 1957. 48*s. net.*

The study of thyroid hormone metabolism is being pursued so actively and in so many laboratories that it is unusually difficult to obtain a survey of the subject which is both comprehensive and up to date. A Ciba colloquium was held in June 1956 at which the control of thyroid hormone release and the biochemical nature of the hormones' action and metabolism were discussed with the fullness, freshness, and authority characteristic of these meetings. The papers and their discussion as now published are outstandingly valuable for the review that they give of this rapidly developing subject. As one would expect, this is a collection of ideas, suggestions, and comments as well as a very full survey of results; the discussions following each paper preserve their verbatim form, although sufficiently edited by their authors to appear articulate in print. The biochemistry of these hormones, their possible active forms, and their renal and hepatic metabolism are particularly well and authoritatively dealt with in papers discussed or presented by Pitt-Rivers, Roche and Michel, Thibault, Gross, Taurog, Maclagan and Wilkinson, Lardy, Vannotti, and Lissitzky: names which in themselves indicate the value and quality of this aspect of the colloquium. The papers of Halmi, Barker, Querido, and Courrier all also contain essentially new points of biochemical importance in thyroid metabolism; the pituitary control of the gland's activity is the subject of further papers given by Purves, Greer, and G. W. Harris. For those working on the biochemistry of the thyroid this book will be essential, and it is likely to form a useful and suggestive survey for those working in many fields of metabolism and of endocrine function.

E. E. POCHIN

NUCLEIC ACIDS

Biochemical Society Symposia No. 14: Structure of Nucleic Acids and their Role in Protein Synthesis, *edited by E. M. Crook. Pp.* 74. *Cambridge University Press, London.* 1957. 20*s. net.*

This is the collection of papers presented for a Biochemical Society symposium in London in February 1956. The collection reviews and discusses the evidence for the structures of nucleic acids and the role of these acids in protein synthesis. It begins with a brief

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discussion of the recent chemical and physical evidence for the proposed structures of ribonucleic and deoxyribonucleic acids. Cytological aspects and the extensive metabolic evidence for the involvement of nucleic acids in protein synthesis in animal tissues and bacteria are then presented. The collection ends with a discussion of the role of deoxyribonucleic acid in bacteriophage multiplication.

This book summarizes much of this important phase of biochemistry up to 1956 and emphasizes some of the very recent work done in the United Kingdom on these problems. The collection provides a concise, well-presented, and useful review of the interrelation of nucleic acids and protein synthesis and will certainly be of interest to those workers who are turning to these problems. D. GROSS

ENZYMOLOGY

Advances in Enzymology. Vol. XVIII, edited by F. F. Nord. Pp. v + 435. Interscience Publishers Inc., New York; Interscience Publishers Ltd., London. 1957. $9 *net.*

Biochemists who look forward to the annual appearance of volumes of authoritative and topical reviews of various aspects of enzymology and related subjects will not be disappointed with this most recent addition to the series.

This volume contains two excellent complementary reviews on 'Cytochrome in Higher Plants' by E. F. Hartree and on 'Reaction Paths in the Respiration of the Higher Plants' by W. O. James. Though there is some unavoidable overlap between these two chapters, readers wishing to acquire up-to-date detailed knowledge of interesting progress in plant biochemistry will be glad to have the two points of view in one volume.

A. Kornberg's article, 'Pyrophosphorylases and Phosphorylases in Biosynthetic Reactions', gives a wealth of information about reactions of many enzymes with phosphate esters, including those involved in *in vitro* synthesis of nucleic acids.

R. A. Peters, in 'Mechanism of Toxicity of the Active Constituents of *Dichapetalum cymosum* and Related Compounds', describes an interesting chapter in biochemistry—the effect of fluoroacetate on cell metabolism—to which he himself has contributed so much. 'Newer Knowledge of Succinic Dehydrogenase' by T. P. Singes and colleagues is timely and valuable, as is 'The Chemistry and Function of Lipoic Acid' by L. J. Reed. An interesting article, *Le rôle biosynthétique du cycle des acides tricarboxyliques* by J. M. Wiame, and reviews on 'Deoxyribonucleoproteins' by J. A. V. Butler and P. F. Davison and on 'Lignification' by W. J. Schubert and F. F. Nord complete this valuable and well-produced volume. H. GUTFREUND

GROWTH SUBSTANCES

Symposia of the Society of Experimental Biology, No. 11: The Biological Action of Growth Substances, *edited by H. K. Porter. Pp. vi + 344. Cambridge University Press, London.* 1957. 55*s. net.*

This symposium is concerned with the physiology of growth substances in plants and animals; their chemistry and biochemistry received only incidental attention. On the botanical side, in particular, it can be regarded as somewhat of a landmark, since it closely followed a period of exciting new discoveries. The spotlight naturally falls on the plant cell-division factors or kinetins, and on the gibberellins with their astounding modifications of plant form. It was also a time for the exposure of old myths, and one must thank Dr J. R. Raper for his masterly exorcism of Moewus's elaborate system of algal sexual hormones that has bedevilled botanists for so long. The current theories of apical dominance and phototropism also come in for some healthy criticism by Gregory and by Brauner respectively. The volume contains a number of less exciting yet nevertheless very valuable summaries of current views of the part played by hormones in plants. The papers on the animal growth substances do not evoke the same appeal of new discovery, although they are extremely valuable statements of the present picture in its many aspects. Particularly readable is the account by Wigglesworth of growth hormones in insects and the discussion by Klein and Klein of the evolution of independence of mammalian tumour cell populations.

The production of the volume is of the technical excellence we have long come to associate with the symposia of the Society of Experimental Biology. This new volume contains such a wealth of ideas and information that it will be indispensable to all workers in this and associated fields. L. J. AUDUS

VISUAL PIGMENTS

The Visual Pigments, *by H. J. A. Dartnall. Pp. vii + 216. Methuen & Co. Ltd., London; John Wiley & Sons Inc., New York.* 1957. 30*s. net.*

The visual pigments occur in the retina of the eye, and their light sensitivity is basic to vision. This book is a critical survey of the experimental methods used, the results obtained, and the theories put forward in the study of the nature of the pigments and their reaction with light. A large part of the book is concerned with visual purple (rhodopsin), which is a compound of protein with a derivative of vitamin A and is the best-known light-sensitive pigment of the vertebrate retina. After a welcome introductory chapter describing experimental methods there are chapters on the visual pigments and their photoproducts, on the physical and structural chemistry of visual purple, and on the part played by different stereoisomers of vitamin A in the photochemistry of the different pigments. These are followed by chapters in which the methods of assessing homogeneity of visual pigment solutions are discussed, and the book ends with a description of experimental work carried out on visual pigments in the retina rather than after extraction into solution. This is the first comprehensive book on the visual pigments for a very long time, and the author is to be congratulated on his balanced account of the subject. He has not over-simplified the evidence and leaves us with the feeling that though a great deal is now known, there are still many uncertainties, and generalizations may be premature. A. PIRIE

PLANT PATHOLOGY

Plant Diseases due to Bacteria (*second edition*), *by W. J. Dowson. Pp. xv + 232. Cambridge University Press, London.* 1957. 32*s.* 6*d.*

Until the first edition of this work appeared in 1949 there was a noticeable gap in the field of plant pathology covered by British textbooks and manuals. The publication of this second edition after as short an interval as seven years justifies the confidence felt by competent judges as to the high standard of the work and the wide need for the book by research and advisory plant pathologists as well as by students of biology and bacteriology. The subject matter has been generally brought up to date, and ample

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reference is made to investigations into the etiology and control of bacterial diseases of crop plants carried out since 1949. Care has also been taken to include the latest microchemical tests used in determinative bacteriology, and mention is made of the use of specific phages for bacterial identification. While the format of the work remains almost unchanged, a short new chapter on the preservation of bacterial cultures has been added. The photographs, although reduced in number from forty to thirty, are ample and well chosen, while the re-inclusion of the series of crop disease distribution maps published by the Commonwealth Mycological Institute is a welcome feature. The book is very well produced, and there are few errors. A. E. MUSKETT

MAMMALIAN DEVELOPMENT

Parthenogenesis and Polyploidy in Mammalian Development, *by R. A. Beatty. Pp. xi + 132. Cambridge University Press, London.* 1957. 15*s. net.*

There is no doubt that two fields of nquiry to which biologists nowadays are giving increasing attention are the detailed cytology of early development and the study of spontaneous or induced irregularities in chromosome number. That these apparently unrelated subjects are in fact truly complementary and gain much in comprehensibility when considered together is amply demonstrated in Dr Beatty's book. The author achieves a notable synthesis, setting out clearly the genetic consequences of the modifications that can occur during the initiation of a new individual. Variation arises in different ways: through heteroploidy of spermatogonia or oogonia, through failure of abstriction of either or both polar bodies, through abnormalities of fertilization and cleavage, or through non-specific activation of the egg. Since development may proceed under one or more of these circumstances, numerous possible combinations exist, and the multiplicity of results would become hopelessly confusing were it not for the skilful and systematic manner in which Dr Beatty handles his material. A large volume of published data, from non-mammalian as well as mammalian species, is critically examined and classified, and the text thus provides a unique source of evaluated information. Several significant implications are discussed, and the incisive conclusions given add further to the value of this important little treatise. C. R. AUSTIN

ENTOMOLOGY

A General Textbook of Entomology *(ninth edition), by A. D. Imms, revised by O. W. Richards and R. G. Davies. Pp.* x + 886. *Methuen & Co. Ltd., London.* 1957. 75*s. net.*

This book was first published in 1925 and the last complete revision was made in 1934. To bring the work up to date while retaining its general form unchanged was a formidable task which has been magnificently accomplished by Professor O. W. Richards and Dr R. G. Davies. The whole book has been expanded by about one-quarter. The number of orders recognized has increased from 23 to 29. Classification at the family level has been extensively revised in almost all groups, and new keys for the identification of families have been prepared. Many new references have been added, and these are now set out in a standard and extended form at the end of each chapter. The physiological sections did not really represent current knowledge, even in 1925; they have now been completely rewritten and provide a condensed and accurate account of present ideas on the physiology of insects. Since its first appearance 'Imms' has held its position as the most useful general account of entomology. In its new form it is still further improved.

V. B. WIGGLESWORTH

APES AND MAN

Primates, Part 3—Pithecoidea, Platyrrhini, Hapalidae, *by W. C. Osman Hill. Pp. xix* + 354. *Edinburgh University Press, Edinburgh.* 1957. 90*s. net.*

With this third volume Dr Hill has arrived taxonomically roughly at the half-way stage of his monumental survey of the Primates. He begins here his review of the higher suborder of the monkeys, apes, and man, which collectively he calls the *Pithecoidea*, though what is to be gained by a resurrection of this term in place of the currently accepted *Anthropoidea* is by no means clear.

In accordance with his general plan for the whole series Dr Hill provides descriptions at each hierarchical level, working down from the suborder *Pithecoidea* (= *Anthropoidea*) and the infra-order *Platyrrhini* to the families and the various genera of the Marmosets and Tamarins. This arrangement imposes some repetition, but in practice it is often of great value to have these comprehensive definitions without recourse to an inordinate amount of cross-reference. Indeed, Dr Hill has done his work conspicuously well in the way these taxonomic descriptions have been dovetailed.

The present volume will be especially welcomed, as there has long been a noticeable lack of good systematic generic descriptions of the New World monkeys. One cannot readily agree, however, with the taxonomic and nomenclatural changes which Dr Hill continues to introduce in his survey. His discussions are not always sufficiently critical to persuade the reader to accept many of his rearrangements. Despite these reservations, there is no doubt that Dr Hill's work has already an assured place as an indispensable guide to the morphology and distribution of the primates. J. S. WEINER

MEDICINE AFLOAT

Medicine and the Navy 1200–1900, *by J. J. Keevil. Vol. I,* 1200–1649. *Pp. xii* + 255. *E. and S. Livingstone Ltd., Edinburgh.* 1957. 40*s. net.*

Under the terms of their trust, the Wellcome Trustees are empowered to give assistance 'for the collection of information of every kind connected with the history of medicine and allied sciences.' In sponsoring the present work they are using their powers to admirable purpose, for—strange as it may seem, especially to a seafaring nation—no systematic study has previously been made of the part played by medicine in the lives of those who go down to the sea in ships. Surgeon Commander Keevil is well equipped to provide such a study, and in this volume, the first of four, he treats of the early period from 1200 to 1649. Starting with the introduction of the laws of Oléron into England at the opening of this period, he goes on to describe—among much else—the provision made for sick and injured seamen, the *ad hoc* engagement of surgeons for service 'with the king's lieges sailing . . . to Calais' in 1436, the ravages of disease in both the Spanish and the English fleets at the time of the Armada, and the large share of responsibility borne by sickness for the naval decline that followed the first Elizabethan age.

The royal ships were in fact sadly neglected from the medical point of view, and the author stresses the fact that the origins of naval medicine are mainly to be found in the voyages of privateers and merchantmen. Such perspicacious adventurers as Drake

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gradually acquired considerable knowledge of the conditions necessary for healthy life at sea, together with experience of tropical diseases, and in due course the importance of having competent medical help on board won tardy recognition. Even by 1649, however, the state of affairs was very far from satisfactory, and we must await Mr Keevil's subsequent volumes to learn how it was improved. This first instalment of the work is absorbingly written, fully documented, and well illustrated. Most appropriately, it is dedicated to Charles Singer. E. J. HOLMYARD

PLANT BREEDING

Plant Breeding and Genetics in India, *Vol. I, by R. H. Richharia. Pp. vii + 410. Scientific Book Co., Patna, India.* 1957. 30*s. net.*

This book, with slight modifications, is a reprint of a work with the same title published in 1945. In it a survey is made of the considerable literature on plant breeding and genetics, embracing the principal economic crops of India. It deals only with the researches of workers in India, and in this respect is exceptionally complete from the bibliographical point of view. There is, however, no reference to the studies of workers in other countries, although such studies have often covered the same ground and arrived at the same conclusions. It is thus difficult for either Indian or foreign readers to tell where foreign work left off and Indian work began, and vice versa, and considerable historical confusion results.

The information provided, for example, on the genetics of *Ricinus* with reference to the work of Patwarden on capsule spininess and waxiness was anticipated many years previously by work in the West Indies and the United States. Surely Indian workers should know this. It may be true that the haploid number of chromosomes of the garden pea was determined by Roy in 1933 as 7, but this fact was common knowledge many years previously.

The author has nevertheless assembled a large amount of useful information, and has done service both to his Indian colleagues and to foreign workers. It is his intention to publish a further volume dealing with work done in India after 1945. S. C. HARLAND

WILLIAM HARVEY

William Harvey. His life and times; his discoveries; his methods, *by Louis Chauvois. Pp.* 271. *Hutchinson Medical Publications, London.* 1957. 25*s. net.*

Around the rather thin materials for the life of Harvey, Dr Chauvois, a well-known French historian of medicine, has woven a fresh narrative of the career and discoveries of the great physiologist. Excellently translated into English and amply illustrated, it is a useful and interesting volume; even the author's excursions into purely imaginary incident are made tolerable by his obvious enthusiasm. Earlier researches by D'Arcy Power, H. P. Bayon, Geoffrey Keynes, and others, which have brought to light almost everything that is known of Harvey as a man and fully investigated his stature as a scientist, have been skilfully used. Dr Chauvois' most abrupt dissent from the views of these authorities is in the importance he attaches to Cesalpino as a precursor of Harvey. In Cesalpino's writings he finds 'set forth quite clearly a circulation from arteries to veins and then from veins to arteries', and though Dr Chauvois admits that Harvey nowhere refers to Cesalpino, his 'reconstruction' supposes that Harvey could have derived his idea from the Italian naturalist. The extensive quotations adduced to support this opinion do not seem as clear as the author's interpretation of them. Despite this, however, Dr Chauvois sets Harvey's achievement at the highest level, and justly explains its fundamental significance for medical science. His biography can be commended to all whose interest has been aroused by the recent celebrations, though for the scholar it will hardly replace the older books. A. R. HALL

HISTORY OF NAVIGATION

The Haven-Finding Art. A History of Navigation from Odysseus to Captain Cook, *by E. G. R. Taylor. Pp. xii* + 295. *Hollis and Carter, London.* 1956. 30*s. net.*

The myth that early sailors navigated by hugging the shore dies hard, even though, as Professor Taylor points out, 'every sailor has a wholesome dread of being driven on to a lee-shore and stands well out to sea to avoid the dangers . . . of inshore waters.' She amasses ample evidence that long before the Christian era, when the sounding-line was the sole instrumental aid to navigation, sailors sought the relative safety of the open sea and regularly ventured far out of sight of land. In recounting that Odysseus sailed by the stars, Homer was evidently referring to a practice even then quite familiar to sailors.

From these early beginnings she tells the story of how men navigated at sea until the time of Cook, perhaps the greatest navigator of all time. She describes not only instrumental aids—ranging from the *kamal* and traverse-board to the mariner's compass and Harrison's chronometer—but such other related topics as charts, pilot books, and tide-tables. All this is combined with a very clear account—written with remarkable insight into the seaman's problems—of how the instruments were, and in many instances still are, used at sea. This is an exceptional book: at once both a work of serious scholarship and a very readable account of a subject of great interest and importance.

TREVOR I. WILLIAMS

RENAISSANCE SCIENTISTS

Six Wings: Men of Science in the Renaissance, *by George Sarton. Pp. xiv* + 318. *Indiana University Press, Bloomington.* 1957. $6.75 *net.*

This is a study based upon the late George Sarton's Patten Lectures delivered at Indiana University in 1955. The 'six wings'—a scholarly allusion to the six parts of the astronomical tables of the medieval writer Immanual Bonfils, and thence to the six wings of the seraphim in the Book of Isaiah—are the divisions of the book, which in characteristic fashion ranges over all parts of Renaissance science, medicine, and technology. The emphasis is upon the personalities involved rather than upon the investigations they made: in pursuance of this aim the book is illustrated with a considerable number of contemporary portraits. To say that the whole is erudite but eminently readable, far-ranging but not desultory, profound but never pretentious, is superfluous when we refer to Sarton: it is sad indeed that so powerful an intellect is with us no more. Among so much that is good it is invidious to praise any feature in particular, but many will find special enjoyment in the final 'wing', which is a brilliant comparative study of Leonardo da Vinci and Albrecht Dürer. Clearly, Sarton felt spiritual affinity with Leonardo: 'Dürer was always practical and earth-bound; Leonardo was the opposite . . . ; I admire them both but I love Leonardo.'

TREVOR I. WILLIAMS

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Short notices of books

(These notices are descriptive rather than critical and are designed to give a general indication of the nature and scope of the books.)

A Guide to the Literature of Chemistry (*second edition*), *by E. J. Crane, Austen M. Patterson, and Eleanor B. Marr. Pp. xiv* + 397. *John Wiley & Sons Inc., New York; Chapman and Hall Ltd., London.* 1957. 74*s. net.*

A proper understanding of how and where to seek information in the vast literature is of the greatest importance to all chemists. Since 1927 Crane and Patterson's 'A Guide to the Literature of Chemistry' has been a standard work. It now appears in a revised and modernized form, the revision being largely the work of Dr Eleanor B. Marr. It is an effective guide to books, journals, patents, government publications, trade literature, libraries, and other sources of chemical information.

Dictionary of Scientific Terms (*sixth edition, revised and enlarged by John H. Kenneth*), *by I. F. Henderson and W. D. Henderson. Pp. xvi* + 532. *Oliver and Boyd, Edinburgh.* 1957. 32*s. net.*

The scope of this work is considerably less than its title suggests, for it is in fact a dictionary of terms used in the biological sciences. Within these limits it ranges widely, and some fourteen thousand terms are defined. This is a well-established reference book, for the first edition, prepared by I. F. and W. D. Henderson, was published in 1920 and was itself somewhat on the lines of Stormonth's 'Manual of Scientific Terms', which first appeared in 1879. The present (sixth) edition, prepared by J. H. Kenneth, represents a substantial revision and expansion of the last (1953) edition.

The Science Museum: the First Hundred Years. *Pp.* 85. *Her Majesty's Stationery Office, London.* 1957. 15*s. net.*

The Science Museum at South Kensington, one of the fruits of the Great Exhibition of 1851, was first opened to the public in 1857. This book has been published to mark the centenary. A short introduction recounts the history of the museum, and the remainder of the book describes and illustrates twenty-one of its principal exhibits. The latter range from the famous Wells Cathedral clock of 1392 to Parsons' steam turbine of 1891; from Boulton and Watt's rotative beam engine to Joule's apparatus for measuring the mechanical equivalent of heat.

The United Kingdom Contribution to the International Geophysical Year 1957–58. *Pp.* 72. *The Royal Society, London.* 1957. 10*s. net.*

The International Geophysical Year involves the co-operation of some sixty nations. The arrangements for the United Kingdom contribution have been entrusted to the Royal Society, which in this book describes what is to be done. The book describes in some detail the organization of the work, the contributions of the various disciplines involved, and the geographical distribution of the United Kingdom stations. The final chapter describes the establishment and programme of the Royal Society's base at Halley Bay in Antarctica.

New Intermediate Physics, *by G. R. Noakes. Pp. viii* + 961. *Macmillan & Co. Ltd., London; St Martin's Press, New York.* 1957. 30*s. net.*

This is a comprehensive survey of physics designed for students preparing to enter British universities. The sections on general physics and on heat are substantially the same as the author's earlier books on these subjects; the sections on light and on electricity were inspired by earlier books but have been entirely rewritten. In the section on light a wave treatment is used throughout. The M.K.S. system of units is used.

Sztuka Spoleczenstw Paleolitycznych, *by Witold Hensel. Pp.* 158. *Panstwowe Muzeum Archeologiczne, Warsaw.* 1957. 42 *zlotys net.*

This is a short account—handsomely illustrated in colour, half-tone, and line—of the art of palaeolithic communities. It treats the subject from many points of view: the distribution and nature of examples of palaeolithic art, the materials and implements used, the evolution of artistic thought, and the functions of this primitive form of art. The main text is in Polish, but there are short summaries in Russian and English.

Practical Microscopical Metallography (*fourth edition*), *by Richard H. Greaves and H. Wrighton. Pp. xiv* + 221. *Chapman and Hall Ltd., London.* 1957. 70*s. net.*

This is the fourth edition of a standard work on the methods and results of the examination of metals by means of the optical microscope. While this edition follows the same lines as its predecessors, the opportunity has been taken for revision and enlargement.

Men, Missiles and Machines, *by Lancelot Hogben. Pp.* 69. *Rathbone Books, London.* 1957. 17*s.* 6*d. net.*

Power is an overwhelmingly important factor in modern civilization: it has been stated with a good deal of truth that a nation's standard of life is proportional to the amount of power available per head of its population. This beautifully produced and illustrated book—the fifth in a now well-known series—describes for the layman the basic principles governing the production and utilization of power and the various devices that are used for the purpose.

Rectifying Semi-conductor Contacts, *by H. K. Henisch. Pp. xii* + 372. *The Clarendon Press, Oxford; Oxford University Press, London.* 1957. 70*s. net.*

This is a specialized account of the phenomena observed at contacts between metals and semi-conductors, and it includes a detailed description of experimental methods. While a considerable knowledge of physics and mathematics is assumed, the introductory chapters are designed to assist those who have no detailed knowledge of the concepts and terminology of solid-state physics.

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From Dr P. N. Bhandari
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Robert Hare and the oxyhydrogen blowpipe

The attainment of temperatures high enough to melt metals such as platinum and refractory minerals such as alumina is of very great technical importance. Today it presents no great difficulty, and in the thermonuclear field experiment is being directed, as is described elsewhere in this issue, to the controlled attainment of temperatures of the order of many millions of degrees. It is consequently rather easy to forget the difficulties of the pioneers and the extent of their contribution. In this respect the work of Robert Hare, inventor of the oxyhydrogen blowpipe, is of much interest, and it is appropriate to recall the history of this invention on the occasion of the centenary of Hare's death. His methods were crude by modern standards, but from them have evolved many of the modern methods of cutting and welding metals that are of such great practical importance.

The oxyhydrogen blowpipe was the sole source of high temperatures for industrial use until Langmuir devised his atomic hydrogen blowpipe, shortly after his discovery, in 1911, of atomic hydrogen. This blowpipe was used for platinum working and as a source of heat for the limelight that was used in theatres. It was superseded by electrical heating and by the oxyacetylene blowpipe. The latter had the disadvantage of not providing a protective, non-oxidizing atmosphere of hydrogen, but the ease of working given by its higher temperatures (though these were still only of the order of 4000° C) ensured its success, and its use was widely extended and developed during the first World War.

Hare was by no means the first in the field, for many chemists from Lavoisier onwards had appreciated that intense heat could be generated by burning hydrogen and oxygen together. He was, however, the first to evolve a practical apparatus, though the name of Edward Clarke, professor of mineralogy at Cambridge, is often linked with the discovery.

Hare was only twenty years of age when he made his great invention, demonstrating it at Philadelphia to Joseph Priestley, who had settled in the United States after the unhappy experiences at Birmingham that resulted from his sympathy with the French revolutionaries. It consisted of a double platinum jet with converging ducts; these formed the continuation of two tubes of solid silver, the two jets uniting in a common exit just before the orifice. The hydrogen and the oxygen were derived from the electrolysis of water—hence the original name of 'hydrostatic' blowpipe. For reasons of safety—which, as we shall see later, Clarke ignored to his peril—the gases were contained in separate containers. With this apparatus Hare melted a two-pound block of platinum. This he supported on refractory firebricks in such a way that it could be preheated in a conventional furnace, thus conserving the supply of gas. He noted how in this way crude platinum was 'greatly purified' by the volatilization of some impurities in the metal.

Both Hare and Priestley were members of the Chemical Society of Philadelphia, founded by James Woodhouse in 1792 and the oldest chemical society in the world save one founded by Black's students at Edinburgh in 1785. In 1802 this society published an illustrated memoir describing Hare's invention in detail, and he himself described it in a number of American scientific journals. His work was taken up by, among others, Benjamin Silliman at Yale, founder of the 'American Journal of Science', generally known as 'Silliman's Journal'; but the latter introduced no fundamentally new ideas. In a paper on the 'Fusion of strontites and the volatilisation of platinum' Hare described in full his blowpipe and the apparatus made by Silliman for storing the gases in pneumatic troughs.

At Cambridge, Clarke was appointed to the chair of mineralogy in 1808 and carried out various experiments with oxyhydrogen flames. Unlike Hare, he did not use separate vessels to contain the two gases and in consequence became involved in dangerous explosions: on one occasion he was almost killed. After a copper vessel had burst, endangering the lives of two bystanders, a balloon enclosed in protective planking was used as a container. The first hint of Hare's work seems to have reached Clarke through Newman, a London instrument maker, who in 1816 described in the journal of the Royal Institution a blowpipe using air. Clarke looked upon his blowpipe, using 'a highly condensed mixture of the gaseous constituents of water', as a modification of the Newman apparatus. He later admitted Hare's claim to priority, but seems never to have acknowledged it publicly.

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Hare's achievement was recognized by the award of the Rumford medal of the American Academy of Boston, but not until a year before his death in 1858 was the first practical step taken towards making use of the invention for industrial purposes. In 1557 Julius Caesar Scaliger had made the first reference to a South American metal 'which no fire nor any Spanish artifice has yet been able to liquefy'. Exactly three hundred years later, Henri Debray and Henri Ste-Claire Deville announced that they had taken out patents in France, England, and other countries for a platinum-melting technique. They used Hare's oxyhydrogen flame within a small furnace of lime blocks in place of Hare's firebrick refractories. At this time Deville had turned from his brilliant work on aluminium production to the study of the working of such refractory metals as cobalt, chromium, manganese, and platinum. In London the announcement attracted the immediate attention of George Matthey, of the firm of Johnson, Matthey & Co., pre-eminent in the platinum industry. He went immediately to Paris, taking with him some platinum and platinum residues for trial. He found the claims to be justified, and a fruitful collaboration immediately began. In the United States, Joachim Bishop, who had collaborated with Hare in his work on the fusion of platinum, became a founder of the American platinum industry in the firm of Bishop & Co. of Pennsylvania.

The oxyhydrogen blowpipe immediately became a tool of the first importance to chemical engineers, for in platinum apparatus, made possible by Wollaston's perfection of his powder metallurgy technique, it allowed the welding of joints in place of the old gold-soldering. An exhibition of chemical plant staged in London in 1861–62 included a variety of apparatus made by this process as well as a platinum ingot weighing some 450 pounds. Sulphuric acid boilers handling two tons a day of rectified acid were introduced, and these were soon followed by others capable of handling five tons per day. In 1863 Kessler's cascade system for the concentration of sulphuric acid was demanding the manufacture of series of enormous platinum evaporating dishes.

While the blowpipe was Hare's greatest achievement, it ought not to be regarded as by any means his sole claim to fame. His father was English, and had a brewery in Philadelphia; the management of this was his first job, but he soon decided that he wished to make his career in chemistry. Accordingly he joined the Chemical Society of Philadelphia, and doubtless his meeting there with so distinguished a chemist as Priestley was of great help and encouragement to him. He was for nearly forty years, from 1818, professor in the Medical School of the Pennsylvania University, and gained a great reputation both as a lecturer and as a practical man and skilled mechanic. He combined these talents in the construction of apparatus for lecture demonstrations. He carried out many electrochemical experiments, originally using as his source of electricity the same kind of cumbrous troughs, introduced by Cruickshank, as Davy had used in the isolation of sodium and the alkali metals. He soon replaced this by what he called a deflagrator, by means of which any number of zinc-copper couples could at will be brought into action. With this, Silliman succeeded in vaporizing carbon in an arc, and Hare himself used a similar method to fuse platinum. He published an account of the deflagrator as early as 1821 as a 'Memoir on some new modifications of galvanic apparatus', in both the 'Philadelphia Medical Journal' and 'Silliman's Journal', but it attracted little interest. Not until 1835 did it come to the attention of Faraday, then seeking to improve the voltaic battery: he immediately recognized its importance and adopted it.

Hare is also credited with the invention of the first electric furnace, forgotten and re-invented many years later. With it he made calcium carbide, phosphorus, and graphite, all now made industrially in very large quantities in modern electric furnaces. He noted that certain substances, such as lime, which did not fuse in the flame of his blowpipe, glowed very brilliantly when strongly heated. He saw this applied as the Drummond light and in lighthouses. He also introduced the use of a mercury cathode for the electrolysis of solutions of metal salts. Hare corresponded with Faraday, and with Liebig, Berzelius, and other leading chemists of the day.

Like so many others, Hare suffered from living before his proper time. His inventions became of far-reaching importance, and had their exploitation followed close upon their making he would undoubtedly have a far higher reputation as a chemist than is generally accorded to him.

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The Darwin–Wallace centenary

SIR GAVIN DE BEER

The collection of facts has always been an essential part of science, but progress depends in the main upon the interpretation of numerous seemingly isolated facts in terms of some general principle. No generalization is of deeper significance than the theory of organic evolution that was announced jointly by Charles Darwin and Alfred Russel Wallace in July 1858. This theory is the fruit of one of the most intensive searches for facts ever carried out and has in all its essentials stood the test of time and is now universally accepted.

FROM SPECIAL CREATION TO TRANSFORMISM

Only one hundred years have gone by since the concept of evolution was brought to the attention of thinking men in a manner which has compelled its acceptance. The demonstration that the members of the plant and animal kingdoms are as they are because they have become what they are, and that change, not immutability, is the rule of living things, is one of the most important contributions ever made to knowledge, and its effects have been felt in every field of human thought.

That plants and animals constitute natural kinds, or species, had become clear by the end of the seventeenth century, when John Ray defined them as groups of individuals that breed among themselves. In general, species were accepted as being the result of special creation in each case, and there was little incentive to inquire further.

In the eighteenth century doubts began to arise concerning the immutability of species. Some philosophers arguing theoretically, and a small number of naturalists who encountered difficulty in distinguishing between varieties of cultivated plants, and of domestic animals, which were recognized as the diversified products of species, found difficulty in accepting the view that species were unchangeable. Some naturalists, including Linnaeus himself in his later years, adopted a compromise, allowing that species could have descended with modification from genera, but that genera were immutable.

With the increase in detailed knowledge of the flora and fauna of the world consequent upon the final stages of exploration, the problem of the distinction between varieties and species became acute. With boldness, and a breadth of vision amounting to genius, the French naturalist Lamarck cut the knot by proclaiming that there was no essential difference between species and varieties, that both species and varieties were subject to change, and that 'transformism', not immutability of species, was the basis of life. As it happened, there were two reasons why Lamarck's ideas were unacceptable. The first was that he undertook no analysis to provide evidence for his notion of evolution: it flashed across his mind, and he assumed its truth without taking the trouble to prove it. Secondly, he attempted to give an explanation of the causes of evolution which, unfortunately, raised opposition to the acceptance of the concept of evolution itself. He supposed that as a result of new needs experienced by the animal, its 'inner feelings' or subconscious activities produced new organs which satisfied those needs. Not only was such a supposition unacceptable for the solution of the problem of the origin of species of animals, but it was totally inapplicable to plants. On the other hand, Lamarck put forward a view which for a long time was accepted but which is now known to be without foundation, namely that the effects of use and disuse were transmitted by inheritance. There for a time the matter rested.

THE FACT OF EVOLUTION

When Darwin started on the voyage of the *Beagle* in 1831, he had no reason to doubt the immutability of species. The speculations of his grandfather Erasmus counted for nothing with him, because they were not supported by evidence. Those of Lamarck on the causes of evolution had the additional demerit of bringing the subject into disrepute by their fanciful nature. It must be added that in Lyell's 'Principles of Geology', to which Darwin owed so much because of the general background of uniformitarianism in place of catastrophism that it advocated, the possibility of evolution was firmly rejected.

Three sets of observations started Darwin's revolt against the immutability of species. The first was occasioned by his studies of the fauna of the Galapagos Islands, where he found that species of finches differed slightly from island to island,

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while showing general resemblances not only to each other but to the finches on the adjacent mainland of South America. If these species had been separately created, why should there have been such a prodigal expenditure of 'creation' just there; why should geographical propinquity have caused these 'creations' to resemble each other so closely; why, in spite of the similarity in physical conditions between the islands of the Galapagos Archipelago and the Cape Verde Islands, are their faunas totally different, the former resembling that of South America while the fauna of the latter resembles that of Africa?

The second set of observations related to the fact that as he travelled over South America he noticed that the species occupying a particular niche in some regions were replaced in neighbouring regions by other species that were different, yet closely similar. Why are the rabbit-like animals on the savannahs of La Plata built on the plan of the peculiar South American type of rodent and not on that of North America or the Old World?

The third set of observations was concerned with the fact that in the pampas he found fossil remains of large mammals covered with armour like that of the armadillos now living on that continent. Why were these extinct animals built on the same plan as those now living?

On the view that species were immutable and had not changed since they were severally created, there was no rational answer to any of these questions, which would have had to remain as unfathomable mysteries. On the other hand, if species, like varieties, were subject to modification during descent and to divergence into different lines of descent, all these questions could be satisfactorily and simply answered. The finches of the Galapagos resemble each other and those of South America because they are descended from a common ancestor; they differ from one another because they are each adapted to modes of life restricted to their own particular island, one for instance feeding on seeds on the ground and another on insects in trees. The volcanic nature and physical conditions of the Galapagos Islands resemble those of the Cape Verde Islands, and yet the Galapagos birds all differ from the birds of the Cape Verde Islands: therefore it is not the physical conditions of the islands that determine their differences. These differences arose because the Cape Verde Island birds share a common ancestor with the birds of Africa, whereas the Galapagos birds share a common ancestor with those of South America. The hares of South America are built on the South American rodent plan because all South American rodents are descended from a common ancestor. The fossil *Glyptodon* resembles the living armadillos because they also share a common ancestor; this case is particularly important because, if living species show affinity with extinct species, there is no necessity to believe that extinct types of animals have left no living descendants. They may have representatives alive today, and this means that the whole wealth of the palaeontological record of fossils is available as material for the study of the problem of evolution.

In possession of a working hypothesis that species have undergone evolution and successive origination by descent, with modification, from ancestral species shared in common with other species, Darwin next proceeded to search the whole field of botanical and zoological knowledge for evidence bearing on his hypothesis. He realized that no general principle that explained the evolution of animals was acceptable unless it also applied to plants. The result was one of the most remarkable attacks on a problem ever made by the inductive method of searching for facts, whatever their import might be.

In the first place, in cultivated plants and domestic animals such as the dahlia, the potato, the pigeon, and the rabbit, a large number of varieties have in each case been produced from a single original stock. Descent with modification and divergence into several lines is therefore certainly possible within the species.

Comparative anatomy reveals the existence of similar plans of structure in large groups of organisms. Plants may have vegetative leaves, and in some cases these are modified into parts of flowers. Vertebrate animals have forelimbs that may be used for walking, running, swimming, or flying, but in which the various parts of the skeleton correspond, bone for bone, from the upper arm to the last joints of the fingers, whether the animal is a frog, a lizard, a turtle, a bird, a rabbit, a seal, a bat, or a man. This is what is meant by saying that such structures are homologous, and these correspondences are inexplicable unless the animals are descended from a common ancestor. Fundamental resemblance is therefore evidence of genetic affinity.

The study of comparative behaviour proves that related forms show gradations in their instincts, such as shamming death in insects and nest-building in birds. At the same time, related species inhabiting different parts of the earth under very

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different conditions retain similar instincts. Examples are the habit of thrushes in England and in South America of lining nests with mud, and that of wrens in England and North America of the males building 'cock-nests'. Why should this be, unless the different species of thrushes and wrens are descended from common ancestors in each case?

Embryology reveals remarkable similarity in structure between young embryos of animals which in the adult stage are as different as fish, lizard, fowl, and man. This similarity even extends to such details as the manner in which the blood-vessels run from the heart to the dorsal aorta, a plan which is of obvious significance in the case of the fish that breathes by means of gills, but not so obvious in that of lizard, chick, or man, where gill-pouches are formed in the embryo but soon become transformed into different structures, and breathing is carried out by other means. This similarity between embryos is explained by the affinity and descent from a common ancestor of the groups to which they belong.

Embryology also provides evidence of vestiges of structures which once performed important functions in the ancestors but now either perform different functions or none at all. Examples of such organs are the teeth of whalebone whales, the limbs of snakes, the wings of ostriches and penguins, and the flowers of the feather-hyacinth. Since Darwin's time countless other examples have been discovered. The most striking of these are the pineal gland which is a vestigial eye, and vestiges of the egg-tooth still found in marsupials, although it is 75 million years since their ancestors had to use an egg-tooth to crack the shell and hatch out of their eggs. Here again, descent from common ancestral forms explains all these cases.

Knowledge of the fossil record in Darwin's time was so imperfect that nothing was then available in the way of series illustrating the course of evolution. Nevertheless, he noticed that in Tertiary strata, the lower the horizon the fewer fossils there were belonging to species alive today. Palaeontology therefore showed that new species had appeared and old species become extinct, not all at the same time, but in succession and gradually. Why should this be so unless new species have come into existence from time to time by descent with modification from other species?

Plants and animals are classified according to their resemblance, and they are placed in one or other of a not very large number of groups, such as ferns, conifers, molluscs, or mammals. But within each of these groups there is subdivision into other smaller groups, mammals being so subdivided into rodents, carnivores, ungulates, and primates for example. Within these again there is further subdivision, and the important point to notice is that classification always places species in groups that are contained within other larger groups. This is such a commonplace that its significance is often overlooked. Why do organisms have to be classified like this? Why are they not strewn in single file up the ladder of the plant and animal kingdoms, or fortuitously like pebbles on a beach, or arbitrarily like the stars in imaginary constellations? The reason is that the arrangement of groups within groups is a natural classification reflecting the course of evolution. It is the result of descent from common ancestors and an indication of affinity; the differences between the groups are due to modification and divergence during such descent.

Darwin also investigated the problem of interspecific sterility and saw that it was by no means absolute, because numerous examples can be found of different species that produce hybrids, and in some cases these hybrids are themselves fertile. From the point of view of breeding, therefore, such species behave like varieties. Why, then, can species not have originated as varieties, by descent and modification from other species?

From the evidence provided by all these sources Darwin built up an irrefutable argument that species have changed and originated from other species and that evolution has occurred. That he should have been able to do so from such few data is a mark of genius, for at the time when he worked out his conclusions, none of the cases had been discovered which would now be used as the most striking examples with which to illustrate the fact and the course of evolution. Chief among these are the beautiful series of fossils which reveal the evolution of the ammonites or of the horses, step by step, and those which represent the precursors of the various classes and groups of vertebrates such as *Archaeopteryx* or *Pithecanthropus*.

The main steps in Darwin's proof of the fact of evolution were established by 1842, when he committed them to paper in the form of a Sketch which he expanded into an Essay in 1844, though neither was published by him. Soon after this, another naturalist, Alfred Russel Wallace, was led to explore similar lines of research. From some simple observations on the distribution of organisms, both geographically over the world and geologically in the fossil record, Wallace drew some equally simple conclusions that are of great

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importance in the history of thought that led to the realization of evolution. They show that, independently of Darwin and in complete ignorance of his work, Wallace had hit upon the same solution of the problem of the mutability of species.

Wallace's observations were based on the facts, firstly, that large systematic groups such as classes and orders are usually distributed over the whole of the earth, whereas groups of low systematic value such as families, genera, and species frequently have a very small localized distribution. Secondly, 'when a group is confined to one district, and is rich in species, it is almost invariably the case that the most closely allied species are found in the same locality or in closely adjoining localities, and that therefore the natural sequence of the species by affinity is also geographical.' Thirdly, in the fossil record large groups extend through several geological formations, and 'no group or species has come into existence twice.'

The conclusion which Wallace drew from these observations was that 'Every species has come into existence coincident both in space and time with a pre-existing closely allied species.' Thought out about 1845, written at Sarawak in 1855, and published in the same year, Wallace's theory already allowed him to say that 'the natural series of affinities will also represent the order in which the several species came into existence, each one having had for its immediate antitype a closely allied species existing at the time of its origin. It is evidently possible that two or three distinct species may have had a common antitype, and that each of these may again have become the antitype from which other closely allied species were created.'

With the help of this principle, in which it is only necessary to substitute 'ancestor' for 'antitype' for the formulation of evolution to be complete, Wallace showed that it was possible to give a simple explanation of natural classification, of the geographical distribution of plants and animals, including those of the Galapagos Islands, of the succession of forms in the fossil record, and of rudimentary organs which would be inexplicable 'if each species had been created independently, and without any necessary relations with pre-existing species.'

So much of the credit for the establishment of the fact of evolution has, rightly, been accorded to Darwin that it is only just that Wallace's contribution to this problem should be recognized and honoured.

The evidence on which Darwin and Wallace based their demonstration that evolution was a fact is not only valid to this day, but has been confirmed in all the branches of science concerned as well as in many new fields. There was in their day not even an inkling of the possibilities of research opened up by comparative physiology and biochemistry, or of serology as a quantitative indicator of the amount of divergence that has taken place between related forms. Why should the chemical substance involved in the mechanism of muscular contraction in most invertebrates be arginine, whereas it is creatine in vertebrates and echinoderms, which on independent evidence are regarded as related? Why should serum immunized against man give precipitations of 64 per cent when mixed with blood of a gorilla, but 42 per cent with that of an orang utan, 29 per cent with that of a baboon, and only 10 per cent with that of an ox? Why should syphilis attack the chimpanzee more seriously than the orang-utan, and the latter more seriously than the baboon? Why should the human ABO blood group system also be found in the apes? The answer to all these questions is that the organisms concerned have undergone evolution from common ancestors, as a result of which members of the various lines of descent share not only structural, mental, and genetical characters, but also physiological and biochemical mechanisms and immunological reactions.

THE MECHANISM OF NATURAL SELECTION

Although Darwin already knew in 1837 that evolution was an inescapable conclusion to be drawn from the evidence, he did not allow himself to proceed any further with his discovery until he had found an explanation of the fact of adaptation. In a general way, all plants and animals are adapted to their environment, for otherwise they could not live. A man drowns in the sea; a fish dies out of water. But there are some structures which show a particularly intimate relationship between the organism and its conditions of life. Mistletoe is a parasite that requires a tree of certain species to live on, a particular insect to pollinate its flowers, and a thrush to eat its berries and deposit its seeds on branches of the same species of tree. A woodpecker has two of its toes turned backwards with which it grips the bark of a tree; it has stiff tail-feathers with which it props itself against the tree; it has a very stout beak with which it bores holes in the tree trunk; and it has an abnormally long tongue with which it takes the

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grubs at the bottom of the holes. Other plants than mistletoe and other birds than woodpeckers do not have all these adaptations, and therefore, if evolution has occurred, it is necessary to give an objective explanation of how these adaptations arose.

Darwin knew that all members of a species are not identical but show variation in size, strength, health, fertility, longevity, instincts, habits, mental attributes, and countless other characters. He soon perceived that such variation could be, and in fact was, turned to good account by man in the course of artificial selection, which he has practised in the production of cultivated plants and domestic animals since the New Stone Age. The key was selection, the practice of breeding only from those parents that possess the desired qualities. But how could selection have operated on wild plants and animals in nature since the beginning of life on earth without man or a conscious being to direct it? The solution of this puzzle occurred to Darwin accidentally when he read Malthus's 'Essay on Population' and realized that under the conditions of competition in which plants and animals live, any variations would be preserved which increased the organisms' ability to leave fertile offspring, while those variations which decreased it would be eliminated. In a state of nature, selection works automatically, which is why Darwin called it Natural Selection.

Darwin was then able to formulate a complete theory providing a rational explanation of the causes as well as of the fact of evolution in plants and animals. It is formally based on four propositions which he already knew to be true, and three deductions which are now also known to be true. They may be enumerated as follows.

1. Organisms produce a far greater number of reproductive cells than ever give rise to mature individuals.
2. The numbers of individuals in species remain more or less constant.
3. Therefore there must be a high rate of mortality.
4. The individuals in a species are not all identical, but show variation in all characters.
5. Therefore some variants will succeed better and others less well in the competition for survival, and the parents of the next generation will be naturally selected from among those members of the species that show variation in the direction of more effective adaptation to the conditions of their environment.
6. Hereditary resemblance between parent and offspring is a fact.
7. Therefore subsequent generations will by gradual change maintain and improve on the degree of adaptation realized by their parents.

This is the formal theory of evolution by natural selection, first announced jointly on 1st July 1858 by Darwin and Alfred Russel Wallace, who had, again independently, come to the identical conclusion. It represents a step in knowledge comparable to Newton's discovery of the law of gravitation.

THE INTEGRATION OF MENDELIAN GENETICS WITH SELECTION

When Darwin wrote, nothing whatever was known about the laws of heredity, and all that he had to go upon was the vague notion that offspring tended to strike an average between the characters of their parents. This supposition went by the name of 'blending inheritance', and it occasioned for Darwin the greatest difficulty with which he had to contend in formulating his theory. In the first place, if blending inheritance were true, it would mean that any new variation which appeared, even if heritable, would be rapidly diluted by 'swamping', and in about ten generations would have been obliterated. To compensate for this it would be necessary to suppose that new variations were extremely frequent. Since whole brothers, sons of the same father and mother, share an identical heredity, any difference between them would have to be due to new variation that had arisen during their own early lives, and variation would have to affect practically all members of a species. This problem of the supply of variation was a difficulty which Darwin felt so acutely that it even led him to look for a source of this supply in the supposed hereditary effects of use and disuse.

This reliance on the effects of use and disuse as a source of variation, without any effect on his main argument, is the only part of Darwin's demonstration that has had to be abandoned, and he would have welcomed the reasons for it. If only Darwin had realized it, the solution to all these difficulties was at that very time being provided by Gregor Mendel, but his results remained unknown until 1900, eighteen years after Darwin's death.

The Mendelian theory of the gene was worked out by T. H. Morgan and his colleagues with an unprecedented wealth of experimental evidence

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from the breeding pen and from cytological studies on the structure of the cell and its chromosomes. It has established, as firmly as Newton's laws of motion or the atomic theory, that hereditary resemblances are determined by discrete particles, the genes, situated in the chromosomes of the cells, which are transmitted to offspring in accordance with the mechanism of germ-cell formation and fertilization, and conform to distributional patterns known as Mendelian inheritance. The researches of C. D. Darlington and others on the structure and behaviour of the chromosomes have reached such a degree of refinement and precision that each step in the mechanism of Mendelian inheritance can actually be seen under the microscope.

The genes preserve their separate identity; they collaborate in the production of the characters of the individual that possesses them, but they never contaminate each other; they remain constant for long periods, but from time to time they undergo a change, known as mutation, which involves a change in the characters which they control; after this they remain constant in their new condition until they mutate again. It has been conclusively proved that the theory of the gene applies to all plants and all animals investigated, and that the mutation of genes is the only known way in which heritable variation arises. The modifications resulting from good or bad food supply, or from the climatic conditions in which plants and animals live, are not inherited and are therefore without significance in evolution.

The history of the reception of Mendelian genetics after its discovery has been peculiar. The earliest mutations discovered, often called 'sports', were usually deleterious and showed marked and discontinuous steps instead of the gradual and continuous variation which Darwinian selectionists looked for as the raw material of evolution. Selectionists therefore rejected Mendelian genetics as the source of variation. On the other hand, the Mendelian geneticists, knowing that their mutations were the only source of heritable variation, thought that as they showed wide discontinuous steps and arose suddenly, ready-made and apparently without long-continued selection, selection was inoperative in evolution, and they rejected it.

With the progress of knowledge it gradually became obvious that each of these two schools of research objected to the other for reasons which were baseless. As more and more genes were identified and their effects studied, it became clear that the wide and discontinuous mutations first observed were the more easily detected extremes of a range in which the majority exert only slight effects. For the same reason, these mutations were deleterious because organisms are delicately adjusted systems, more likely to be upset by large and discontinuous changes than by small and gradual steps.

The Mendelian geneticists also had to learn two lessons. On the one hand they discovered that although individual genes are associated with particular characters, their control of those characters is also affected by all the other genes, which constitute an organized gene-complex. As a result of previous mutations, gene-complexes of plants and animals in nature contain many genes, and these are sorted out and recombined at fertilization in astronomically numerous possibilities of permutations. These recombinations have been shown to bring about gradual and continuous changes in the characters under the major control of individual genes. Sir Ronald Fisher demonstrated the significance of this by showing that a mutant gene that now exhibits the quality known as dominance has gradually become dominant from a previous intermediate condition. This is what has happened to those mutations that confer a benefit on their possessors, and in their case there has been a selection of gene-complexes in favour of those which accentuate the effects of a favourable mutant, so that these effects are manifested even if the mutant gene is inherited from only one parent, which is the definition of dominance. Conversely, with genes that place a handicap on their possessors, there has been a selection of gene-complexes in favour of those which suppress the effects of such genes so that they are manifested only when the mutant gene is inherited from both parents, which is the definition of recessiveness. They may be suppressed even further, as when the effects of such a gene are obliterated and the gene becomes what is known as a 'modifier', without major control over characters. It has even been demonstrated by E. B. Ford, under rigorous experimental conditions, that one and the same mutant gene can be made to become dominant in one strain and recessive in another, simply by selecting as parents those individuals whose gene-complexes accentuate or diminish the effects of the gene.

The second lesson that Mendelian geneticists had to learn was that although the effects of the mutations which they first observed appeared to be clear-cut, they were already the results of past

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gene-complexes. For these mutations have occurred before, and the gene-complexes have become adjusted to them. The fact that a single gene may now act as a switch controlling the production of one or other character-difference does not mean that this character-difference originally arose at one stroke by one mutation of such a switch-gene, because it has probably been built up gradually as a result of past selection in the gene-complex.

It is therefore clear that mutations and recombinations of genes provide the supply of variation on which selection acts to cause evolution exactly in the way Darwin's theory requires. Its requirements are exacting, for, as T. H. Huxley pointed out, some organisms have evolved slowly and others have evolved fast; he saw that natural selection was the only mechanism that could satisfy both those requirements. It is able to do so because Mendelian inheritance is capable of producing both diversity and stability. As Ford has said, an immense range of types must be available for natural selection to act upon, and this is provided by mutation and recombination of genes. Yet when a favourable gene-complex has been achieved it must not be dissipated and broken down, and this is provided against by the facts that the genes do not blend or contaminate one another, and that they mutate only rarely.

THE SIGNIFICANCE OF PARTICULATE INHERITANCE IN EVOLUTION

The particulate theory of inheritance which Mendelian genetics has established involves a number of consequences of fundamental importance for the problem of evolution. In the first place, the substitution of this quantitative and deterministic science for the vague and baseless notion of 'blending inheritance' completely disposes of the difficulty under which Darwin laboured to account for the necessary supply of variation on which natural selection could act. The most characteristic feature of the Mendelian gene is that it never blends, but retains its identity and properties intact for long periods of time until it mutates, after which it remains intact in its new condition until it eventually mutates again. This means that the amount of variation, or variance, present in a population resulting from previous mutations, is not only conserved through generation after generation, but is actually increased as a result of the recombinations of the gene-complexes in their innumerable possible permutations. This power of increase is one of the most important results of the bi-parental method of reproduction and is the reason why organisms possessed of this mechanism have evolved further than those that lack it.

This conservation of variance is to be considered in relation to the rate at which mutation normally occurs. It has been calculated that in organisms as diverse as a bacterium, a maize-plant, a fruit-fly, and in man, any given gene mutates in one in about half a million individuals. It is also clear that this rate is itself the result of selection, and that although seemingly slow, it has been adequate to provide the requisite basic heritable variation which the mechanism of germ-cell formation and fertilization has multiplied, and on which selection has worked to produce whatever evolution has taken place. In other words, mutation not only need not, but must not be more rapid than a slow rate. This rate is ten thousand times slower than what it would have to be if 'blending inheritance' were a fact, and Darwin's difficulty in accounting for an adequate supply of variation is lightened by that amount.

As the originating mechanism for basic heritable variation, mutation has naturally been intensively studied. It has been found that certain physical and chemical agents, including radioactivity, can accelerate the rate at which mutation would naturally occur, but that these induced mutations are similar to those which occur and recur normally, and no correlation whatever exists between the mutagenic agents and the quality or 'direction' of the mutations. Mutations take place with 'blindness and molar indeterminacy', as H. J. Muller has expressed it. This is a finding of capital importance, for it shows that there is no basis for attempts to explain the origin of heritable variation by appealing to environmental factors to evoke appropriate responses, or to the internal factors to make such responses. Nor is there any basis for the view that the environment would evoke appropriate heritable responses if its actions were continued for a sufficient time, because, as J. B. S. Haldane showed, such responses as might be significant in evolution would be detected within the period of the experiment carried out.

In organisms that reproduce by simple division of the whole body, such as bacteria, special conditions apply because reproduction in them involves not only transmission of genetic material in the form of genes, but also transmission of bodily characters, since the latter are carried over wholesale from 'parent' to offspring. Adaptation to new environments can take place in bacteria. Furthermore,

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in bacteria, and perhaps also in higher organisms, it is possible for organic molecules such as bacteriophage particles to enter organisms and become incorporated in the genetic mechanisms so as to behave like genes. These results are full of promise as a field of research into the nature of genes, and perhaps of mutations, but they do not in any way invalidate the principles of Mendelian genetics and inheritance.

Mutations are chemical changes in the gene-molecule, and since chemical stability is not absolute, the puzzle about mutations is not so much that they occur as that they occur so infrequently. This ignorance of the causes which determine the directions in which mutations take place, if such causes indeed exist, is, strange to relate, no handicap to the understanding of the mechanism of evolution, because it is emphatically selection, not mutation, that determines the direction of evolution. This all-important conclusion is based not only on detailed experimental studies on the effects of selection in nature, but also on the demonstration by Sir Ronald Fisher of a general principle. The effects of selection in changing the frequency of genes in a population have been calculated for various percentage benefits in survival-value conferred by such genes. It has been found by calculation that at the observed natural average mutation-rate of one in half a million, no mutant gene has the slightest chance of maintaining itself against even the faintest degree of adverse selection. Furthermore, if the direction of evolution were determined by the direction of mutation, it would be necessary to suppose that such mutations must be predominantly favourable. In fact, the vast majority of mutations have been unfavourable, and natural selection has acted against them by converting the resulting mutant genes into recessives, or by suppressing them into the condition of mere modifiers, or by exacting the more drastic price of abolition consequent on the rapid death of the organisms containing them. It is natural selection, not mutation, that has governed the direction as well as the amount of evolution, and it has been estimated that if mutation were to stop now, there is already sufficient variation in the plant and animal kingdoms for evolution to continue for as long in the future as it has continued hitherto in the past.

The bearing of this demonstration on hypotheses that attempt to explain evolution by postulating the existence of agencies capable of directing mutation is plain. It means that all such theories as invoke the effects of use and disuse, 'inheritance of acquired characters', environmental stimuli, 'organic selection', 'inner feelings', 'inherited memory', momentum along particular directions, orthogenesis, nomogenesis, and others, which assume that mutation can be made to follow adaptively desirable directions, are not only devoid of any known mechanism by which the direction of mutation might be brought about, and devoid of evidence for the existence of such mechanisms, but they involve a cause 'which demonstrably would not work even if it were known to exist'. It is therefore not surprising that in spite of repeated attempts, many undertaken with impure and insufficiently standardized genetic material, and others in which the results were simply faked, no evidence has been provided that the effects of use and disuse or adaptive response to environmental conditions are inherited or induce appropriate mutations. From the evidence provided by genetics, natural selection is the only mechanism capable of explaining evolution.

NATURAL SELECTION, 'IMPROBABILITY' AND 'CHANCE'

An argument sometimes used against the efficacy of natural selection involves the claim that the initial stages in the evolution of complex structures or functions could not have been favoured by natural selection until such structures or functions had reached a certain level of perfection. Like all other arguments of the *non possumus* type, this one melts away before the progress of knowledge. A case in point is that of the electric organs of fish, developed out of muscles which are capable of discharges strong enough to catch prey and defend the fish against its enemies. These organs are clearly adaptive and confer survival-value on their possessors, but the question arises what functions they could perform in the initial stages of their evolution, when it must be supposed that their power was too weak to kill prey or to deter predators. Darwin himself was well aware of this problem, and he met the argument by pointing out that 'it would be extremely bold to maintain that no serviceable transitions are possible by which these organs might have been gradually developed'. He has been proved to be right, because of the discovery by H. W. Lissmann that weak electric discharges given off by certain fish function in a manner analogous to those of radar equipment, and serve to convey information of the proximity of objects in the water. Electric organs can therefore be adaptive even when they are too weak to kill prey or deter predators.

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Another case may be cited because it illustrates the manner in which an adaptive result may be achieved without itself being a direct object of selection. Colour vision has been evolved independently in many groups of animals. Among the light-sensitive elements in the eye, some are specially sensitive in dim illumination; others confer acuteness of vision in bright light when they are individually innervated, with the result that light-stimuli are perceived separately by very small areas of the retina. In each of the two functions of seeing in relative darkness and seeing accurately in the light, increased efficiency confers survival-value from the very start of the improvement. But when both these functions have been achieved in the same eye a mechanism is produced, as E. N. Willmer has indicated, in which the visual elements are differentially sensitive to light of different wavelengths, and this is the basis of colour vision. The emergence of colour vision as an unexpected 'bonus' resulting from the perfection of two other functions is a concrete example of the principle to which Lloyd Morgan applied the term 'emergent evolution'.

It has also been objected that natural selection is a difficult concept to apply to the evolution of very complex adaptations involving co-ordinated variations either in one and the same organism, or even in two different organisms. It is not necessary to go far afield to find examples of this, for in all animals with separate sexes and internal fertilization there has been a separate yet harmonious evolution of the reproductive organs in the two sexes. It has been supposed that such situations argued so high a degree of 'mathematical improbability' that they could not be explained as a result of natural selection, which was, very erroneously, called 'chance'. To this objection there are several answers.

In the first place, those who invoke mathematical improbability against natural selection can be refuted out of their own mouths. Muller has estimated that on the existing knowledge of the percentage of mutations that are beneficial, and a reasoned estimate of the number of mutations that would be necessary to convert an amoeba into a horse, based on the average magnitude of the effects of mutations, the number of mutations required on the basis of chance alone, if there were no natural selection, would be of the order of one thousand raised to the power of one million. This impossible and meaningless figure serves to illustrate the power of natural selection in collecting favourable mutations and minimizing waste of variation, for horses do exist and they have evolved.

It is worth while to study the question of improbability more closely. As Fisher has pointed out, improbability has a different aspect when considered from time before or time after the event. The probability that any man alive today will have sons, grandsons, and successive descendants in the male line uninterruptedly for one hundred generations is infinitesimally small. Yet every man today is the living proof that this contingency, so highly improbable as it may have seemed one hundred generations ago, has nevertheless occurred. Similarly, the effects of natural selection are the reverse of chance when considered *ex post facto*; they are rigorously determined, and what they have done is to channel random variation into adaptive directions and thereby simulate the appearance of purposive change. This is why natural selection has been paradoxically defined as 'a mechanism for generating an exceedingly high degree of improbability'.

Mention of purpose introduces the notion of teleology or fulfilment of design which has sometimes been invoked to explain the production of complex adaptations. Teleology and providential guidance are double-edged weapons with which to attack the problem of evolution, because it can be shown that the more detailed the adaptation, the more 'improbable' it may appear as a product of 'chance', the more likely its possessor is to be doomed to extinction through inability to become adapted to changed conditions.

Structures may be developed which at first benefit individuals in their competition to survive; but by continued selection such structures may become exaggerated and lead to the extinction of the species. This seems to have been what happened to the Huia-bird, where mated pairs constantly remained in company together, and the beaks of the male and female reached an extraordinary disparity of size in adaptation to their very special feeding, but failed to enable the birds to obtain ordinary food when their special diet was unavailable. Excess, even of adaptation, is harmful, and the fossil record shows that the vast majority of lines of evolution have led to extinction, which is a grim comment on the alleged powers of providential guidance and purpose.

From the undoubted fact that many of the products of the plant and animal kingdom convey to man the aesthetic quality of beauty, it has been supposed that beauty is an end in itself to which the criterion of usefulness and survival-value could

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not be applied, and therefore that it could not be imagined as a product of evolution. To this argument Wallace opposed the demonstration that if the quality of beauty were an exception to the principle of evolution by natural selection, it would be necessary to find an explanation for the existence of so much in plants and animals that is positively ugly.

Darwin showed it to be an invariable rule that 'When a flower is fertilised by the wind it never has a gaily-coloured corolla'. The beauty of flowers has been gradually achieved because of the survival-value of cross-fertilization (consequent upon the attraction of insects to such flowers) conferred on plants possessing them. The beautiful colours and structures of birds and some other animals have resulted from the survival-value conferred on successful competitors in sexual selection.

This demonstration of what may be called the natural nature of beauty has been developed still further by Ray Lankester in the course of a soliloquy on alpine flowers: 'All beauty of living things, it seems, is due to Nature's selection, and not only all beauty of colour and form, but that beauty of behaviour and excellence of inner quality which we call "goodness". The fittest, that which has survived and will survive in the struggle of organic growth, is, (we see it in these flowers) in man's estimation the beautiful. Is it possible to doubt that just as we approve and delightedly revel in the beauty created by "natural selection", so we give our admiration and reverence, without question, to "goodness", which also is the creation of Nature's great unfolding?'

In many of the higher animals, parental care and self-sacrifice, in the interest of other members of the family such as incubating or gravid females and young, have been favoured by natural selection and conferred benefit on the species. From earliest human times, the survival-value of altruistic behaviour has been enhanced because of the prolongation of childhood and the consolidation of the family that have characterized the evolution of man. The size of the unit within which altruistic behaviour conferred survival-value has grown progressively larger, but fitfully, as history and anthropology have shown, from the family to the clan, the tribe, and the nation. In this manner, ethical standards of conduct and morality have arisen which can be seen to develop in individuals and have been seen to evolve in societies. Between these units, competition on the subhuman level of natural selection has persisted. With the development of man's higher mental faculties, conscious choice and purposiveness became factors in evolution, and for this reason the subsequent evolution of man has been of a nature different from that of other organisms because it was no longer governed solely by natural selection.

NATURAL SELECTION IN ACTION

Natural selection can be seen to be at work here and now in directing evolution. Modern techniques of study of genetics in populations in the field developed by T. Dobzhansky and E. B. Ford have shown that the relative longevities of variants in different environments can be directly measured, and that the effects of such differential mortality have been to produce evolutionary change. An example of this type of research is that of H. B. D. Kettlewell on 'industrial melanism' in moths (figure 1). Up to 1850 the British peppered moth existed in its typical grey form known as *Biston betularia*, which is remarkably well adapted to resemble the lichens on the bark of trees. From that date a dark melanic variety appeared, known as *carbonaria*, which is extremely conspicuous against the natural bark of trees. The melanic variation is controlled by a single dominant Mendelian gene and is slightly more vigorous than the normal grey type. Nevertheless, because of its conspicuous colour the *carbonaria* variety was constantly eliminated, and this variety persisted in the populations of the peppered moth only because the same mutation kept on occurring again and again. The Industrial Revolution brought about a marked change in the environment, since the pollution of the air by increasing quantities of carbon dust killed the lichens on the trees and rendered their trunks and branches black. Under these conditions it is the *carbonaria* variety which is favoured and the *betularia* penalized. This has been proved by direct observation of the feeding of birds, and by measurement of the survival-rates of the different forms in the different environments. The dark *carbonaria* form survives 17 per cent less well in an unpolluted area and 10 per cent better in a polluted area. One hundred years ago the dark variety of the peppered moth formed less than 1 per cent of the population; today in industrial areas it forms 99 per cent, and selection has made it more intensively black than when it first appeared.

The case of melanism in the peppered moth also introduces a principle to which L. Cuénot drew attention and gave the name of 'pre-adaptation'. The melanic form of the peppered moth happened to be 'pre-adapted' to conditions which were only

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subsequently realized, or in other words, if the Industrial Revolution had not taken place, the melanic variety would never have become adaptive at all, and would have suffered the same fate as the countless other mutations resulting in variations which, whether 'pre-adapted' or not, have been eliminated because they fell short of the requirements imposed by natural selection.

The evolutionary change actually witnessed in the peppered moth is directly attributable to selection, and it is matched by similar studies on other forms. Experiments by A. J. Cain and P. M. Sheppard on the survival-rate of snails with shells of different colours and banding patterns, living on dark- or light-coloured backgrounds, have shown that selection does not act like an all-obliterating steam-roller going in one direction. As the seasons change, the adaptive value of the colour of a shell changes from disadvantageous to advantageous and back again. This proves that the effects of selection vary from place to place and from season to season, and that the balance between an organism and its environment is delicate, changing, and dynamic.

The phenomenon of Batesian mimicry has also been proved not only to be adaptive and to confer survival-value, but to have been achieved by selection. Ford has shown that the degree of perfection with which the mimics copy their models is a function of the prevalence of the models. The percentage of imperfect mimics in the populations of *Papilio dardanus* is only 4 at places like Entebbe, where models are numerous. At Nairobi, on the other hand, where the models are 70 times less numerous than at Entebbe, the imperfect mimics are 8 times more numerous and constitute 32 per cent of the population. Less survival-value is conferred by resemblance to a model when the latter is too infrequent to teach predators to shun it, and there is then less selection-pressure on the mimic to resemble it.

While the overriding importance of the effects of selection is now generally realized, it has been suggested that when populations are split up into very small isolated colonies, changes in the relative frequencies of different genes might result from the errors of random sampling in the formation of the germ-cells and their fertilization, without involving selection. This concept, advanced by Sewall Wright and known as 'random genetic drift', has been invoked as a possible cause of non-selective, non-adaptive evolution. It has, however, been invalidated by the results of experimental studies in the field such as those of Fisher and Ford on moths, which have shown that selective factors are much more important than casual non-adaptive factors in determining the relative frequency of genes and in bringing about close adaptation to local environmental conditions. Even in comparatively numerous populations, from one generation to the next there are fluctuations in gene-ratio larger than can be attributed to random sampling and which are controlled by selection. Such effects as may be due to random sampling in small populations can only be of negligible significance in evolution.

Selection frequently works on a basis of compromise. Among the natives of Africa there is a condition known as sickle-cell anaemia, in which the red blood-corpuscles are deformed and shaped like the blades of sickles. This is controlled by a Mendelian gene which, when inherited from both parents (homozygous), produces an extreme effect which frequently kills the subject by thrombosis. When inherited from only one parent (heterozygous), the danger from thrombosis is not so great. In areas where malaria is present, however, there is a positive advantage in possessing the sickle-cell gene, because the malaria parasite cannot enter the sickle-shaped red blood-corpuscles. In accordance with the prevalence of malaria in the environment, therefore, a balance is automatically struck in the population between the danger of dying from malaria if the individual has no sickle-cell gene, and the danger of dying of thrombosis if the individual has two sickle-cell genes. Survival-value and ability to leave more offspring therefore accrues to the possessors of one sickle-cell gene up to a certain frequency, and this example shows in what unexpected ways selection is able to make the best even of a bad gene-complex.

NATURAL SELECTION AND PALAEONTOLOGY

The palaeontological record provides the evidence of the course which evolution has followed in the past. The fossil material is in places now so rich that it can be used for quantitative studies in evolution. Firstly, the radioactive time-clocks enable various levels of evolutionary lineage to be dated and the time measured during which certain changes have occurred. This provides quantitative evidence of evolution rates. From such data estimates can be obtained of the duration-times of genera and species. Statistical study of large samples of fossil materials enables the variability of the different species to be assessed. By methods

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such as these, G. G. Simpson has worked out that the evolution from *Hyracotherium* to *Equus* occupied 60 million years. This involved passage through 8 genera, the duration of each being on the average 7·5 million years; 30 species of a duration-time of 2 million years each; and 15 million generations each reaching maturity in 4 years. These data can be compared with those obtained from other groups of animals, from which they differ considerably. The results show that evolution rate is not correlated with variability, nor with generation-time, and that it is selection that controls the direction and intensity of evolution.

These results are all the more important because in the past some palaeontologists, unequipped with knowledge of modern genetics, have imagined that from tracing the course of evolution in the lineage of fossils which they established they were in a position to draw conclusions about the cause of such evolution. Some have thought that they had found support for the inheritance of acquired characters, although they knew nothing about inheritance; others imagined that as the lineage of some fossils showed linear progression of certain characters, they were justified in concluding that evolution involved an innate directional component, an expression of 'momentum' leading to evolution in 'straight lines', which they called orthogenesis. They failed to realize that if selection in a particular direction benefits an organism, continued selection in the same direction will, up to a certain point, benefit it further. Others again have concluded from their materials that a distinction in principle could be made out between 'big' evolution leading to large evolutionary changes, and 'small' evolution producing trifling results. None of these speculations can stand up to the evidence that selection determines the course of evolution, its speed or its slowness, the greatness or smallness of the effects produced, and its direction, which if constant for any length of time simulates orthogenesis.

It is of interest to consider how far it is possible to extrapolate the results of modern genetics into the palaeontological past. C. R. Diver has shown that, in snails, the patterns of banding found today were already in existence in Pleistocene times. It is necessary, however, to beware of concluding that because characters are similar they must be controlled by the same genes. Even in one and the same species today, the gene-complex can undergo permutations which reproduce the same structures with different genes. An example is provided by the gene 'eyeless' in *Drosophila* which produces flies with very small or no eyes; this is, of course, extremely harmful. Eyeless flies can, however, be made to breed, and although the mortality is very high, progeny can be reared which after a few generations have eyes like normal flies. In such a stock the 'eyeless' gene is nevertheless present unaltered, as can be proved by mating these flies with normal flies, when the effects of the 'eyeless' gene manifest themselves in all their force, though in the second generation, because 'eyeless' is recessive. This result is therefore in perfect accordance with the principle that Mendelian genes do not become contaminated. What has happened during the inbreeding of 'eyeless' flies is that the reshuffling of the other genes has produced a gene-complex in which the harmful effects of 'eyeless' have been suppressed.

The gene-complex is therefore a dynamic system, as S. C. Harland concluded from his researches on cotton. Genes compete, i.e. are selected in the gene-complex, old genes being dropped and new genes incorporated. During the course of evolution the effective membership of the gene-complex must have changed, and it is not legitimate to conclude that because a character or a structure like the eye of vertebrates was in existence 400 million years ago, it was then controlled by the same genes as control it now. The evidence is entirely opposed to such a static view. It is precisely because the gene-complexes change that characters, structures, and organisms have evolved.

THE NEW SYSTEMATICS AND THE ORIGIN OF SPECIES

The researches on industrial melanism in the peppered moth, banding and colour of snails, mimicry in butterflies, local adaptation in moths, and sickle-cell in man, which have here been briefly described, are examples of new techniques of experimental study of evolution in the field. They have grown out of what Julian Huxley has aptly called 'The New Systematics', to which he has himself contributed so much. Systematics, the study of species and of the higher groups of classification, began by the recognition of differences between species, defined from type specimens preserved in museums. But with the realization that species now or in the past are or were populations of live plants and animals in nature, living under varying conditions of equilibrium with each other and with the inorganic factors of the environments —themselves showing geographical variation in space, and undergoing variation in time, subject to mutation and recombination of their genes,

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FIGURE 1 – *Natural selection in action. Left: Photograph of a tree-trunk from the West of England covered with lichen on which are one typical light-coloured peppered moth (below), and one specimen of the melanic variety* carbonaria *(above). Under these conditions the melanic variety has been observed to fall victim to predatory attacks by birds to a greater extent (17 per cent) than the light-coloured variety. Right: Photograph of a tree-trunk from an industrial region, devoid of lichen and covered with deposits of soot on which are one typical light-coloured peppered moth (above) and one specimen of the melanic* carbonaria *variety (below). Under these conditions the melanic variety has been observed to fall victim to predatory attacks by birds to a lesser extent (10 per cent) than the light-coloured typical form. The melanic variety appeared 100 years ago, and in industrial areas of Great Britain it now forms 99 per cent of the populations of the peppered moth. (From exhibits in the British Museum (Natural History), prepared with the assistance of H. B. D. Kettlewell.)*

constantly under the influence of selection—species can no longer be considered as static milestones of evolution, for they are themselves the dynamic systems by which the roads of evolution are trodden. As genes mutate and are reshuffled, and geographical races invade new ecological niches, advance and retreat, it is already possible on a map to mark out lines of gene-flow, as R. C. Stebbins has suggested from his researches on Californian newts. It may become possible to plot the areas of gene-complex alteration, as can to a certain extent already be done for the origin of cultivated plants such as wheat; but such maps will be continually changing, like the species themselves.

Nobody would have welcomed these developments of biological science more than Darwin himself, as a glance at the last few pages of 'The Origin of Species' will show. It is therefore appropriate to return to the problem with which this article began. As is now certain, species are not immutable but have undergone change, and many examples have been given above. Evolution can take place up to a point without the production of new species, but if this process continues the time must come when new species originate, and it is legitimate to ask whether modern research has revealed any evidence of this. The answer is that new species can be seen originating in nature here and now, and new species have been artificially produced in the laboratory.

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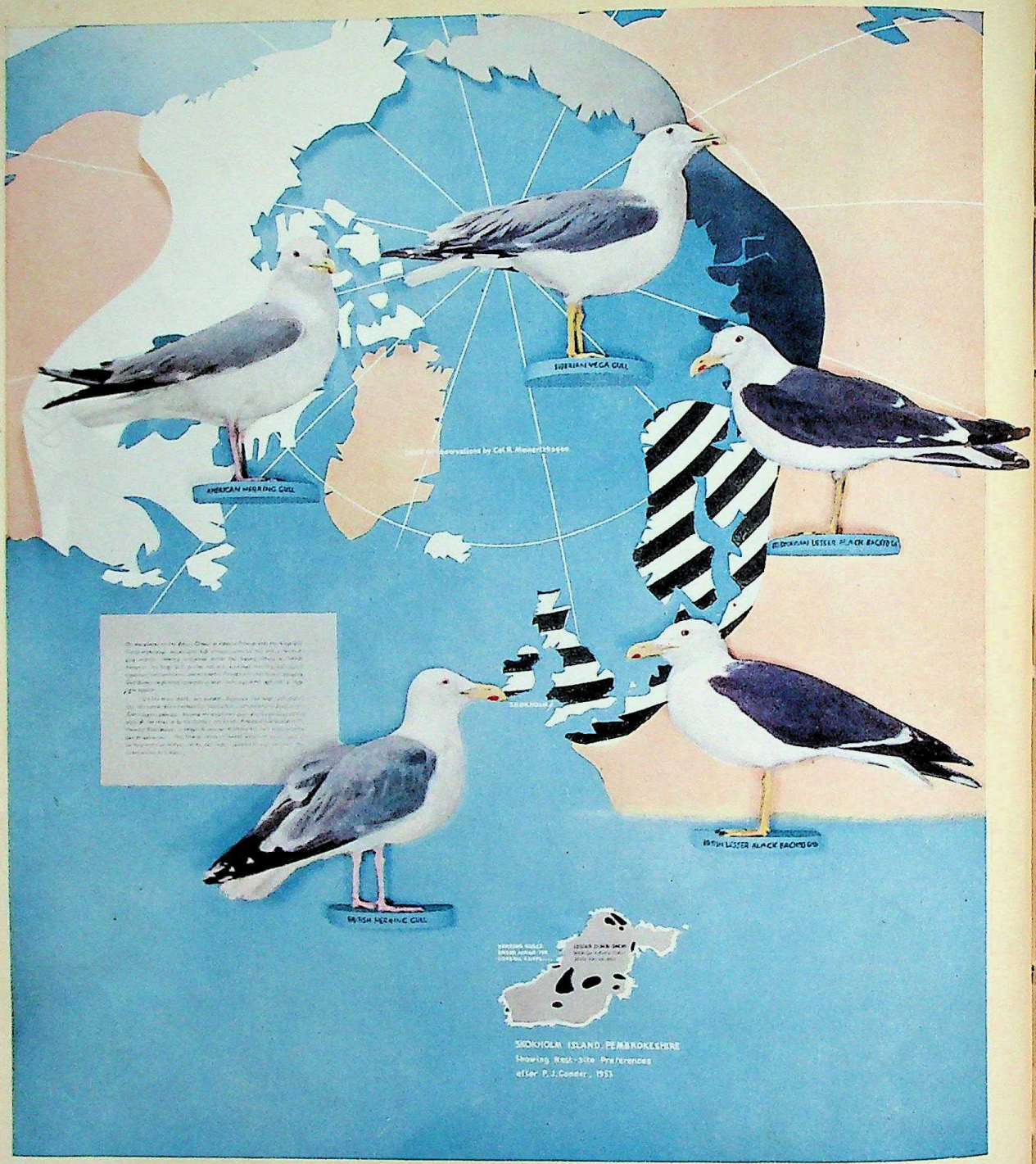

FIGURE 2 – *Geographical species-formation. The British lesser black-backed gull with its dull mantle and its yellow legs grades into the Scandinavian lesser black-backed gull, and it in its turn grades into the Siberian Vega gull with its lighter mantle and its dull flesh-coloured legs. The Siberian Vega gull grades into the American herring gull, which in turn grades into the British herring gull with lighter mantle and pinkish legs. Although the British lesser black-backed gull may be regarded as belonging to the same species as all the other gulls in the chain to the east of it, when it is compared with the other end of the chain represented by the British herring gull, the two may almost be regarded as separate species. They differ not only in colour but in habits, for the former breeds on moors and is migratory in winter, while the latter nests on cliffs and is dispersive. If at any time the chain becomes severed by local extinction of the gull population or the erection of a sterility barrier, the two British gulls will have become separate species. (From exhibits in the British Museum (Natural History).)*

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Speciation takes place when, for various reasons, populations cease to breed with neighbouring populations and, under different conditions of selection, accumulate heritable variations by mutation and recombination of genes in different directions. As E. Mayr has shown, some form of biological isolation between portions of populations is a necessary condition for divergence leading to the formation of new species and higher groups.

Among the kinds of isolation that are chiefly responsible for the origination of species, geographical isolation is the most important; it involves physical barriers such as oceans, mountain ranges, or deserts which separate whole populations. Geographical races are the chief raw materials from which new species are formed, and it was the different finches on the different Galapagos Islands which first suggested to Darwin that evolution had occurred. Here, to various extents, geographical isolation has assisted the origination of a number of species.

A case in which geographical isolation may be expected to produce its effects at almost any moment now is provided by the gulls (figure 2). These birds occupy a zone shaped like a ring round the North Pole and form what B. Rensch has called a chain of races. Starting with the British lesser black-backed gull, with its dark mantle and yellow legs, this is found to grade into the Scandinavian lesser black-backed gull, and, continuing in an easterly direction round the chain, this in turn grades into the Siberian Vega gull with its lighter mantle and dull flesh-coloured legs. The Siberian gull grades into the American herring gull, which in turn grades into the British herring gull, with its light mantle and pinkish legs. Although the British lesser black-backed gull may be regarded as belonging to the same species as all the other gulls in the chain to the east of it, when it is compared with the other end of the chain represented by the British herring gull the two may almost be regarded as separate species. Already they differ not only in colour but in habits, for the latter nests on cliffs and is dispersive in winter, whereas the former breeds inland on moors and is migratory in winter. If at any time the chain becomes severed by the erection of a sterility barrier at any point, either through inability to breed or through a rupture of the chain by local extinction of the gull population, the two British gulls will effectively have originated new species.

Geographical isolation is important for the origin of species of plants as well as of animals, but there is another form of isolation which appears to be restricted to plants and involves the sudden erection of sterility barriers between individuals in the same population as a result of changes in the chromosome mechanism. This is known as genetic isolation. When *Primula verticillata* is crossed with *Primula floribunda*, hybrid offspring are produced, but they are sterile because the chromosomes of one parent species are incompatible with those of the other, and the intricate machinery involved in the formation of germ-cells is thrown out of gear. Occasionally, however, the hybrid plant undergoes doubling of its chromosomes, a condition known as polyploidy, and when that has occurred the hybrid is able to breed with hybrids similar to itself because all the chromosomes have compatible partners, but it is sterile in respect of both parent species. Furthermore, the hybrid is not only true-breeding but is different in structure and in habit from each of its parent species. It therefore fulfils all the criteria of a species and has been called *Primula kewensis*. Many other new species have originated by intentional hybridization and accidental polyploidy in this way. Some of these artificially produced species have been found to be identical with, and to breed with, wild species, and this is the proof that this method of species-formation occurs in nature.

THE CENTENARY OF EVOLUTION BY NATURAL SELECTION

In conclusion, it may be said that during the hundred years that have elapsed since Darwin and Wallace first published their theory, the fact of organic evolution is now universally accepted and its mechanism has been formally explained.

The alternative to evolution is so naïve that it comes as a shock to realize that as recently as one hundred years ago, ideas such as called for the following questions could still be current: 'Do they really believe that at innumerable periods in the earth's history certain elemental atoms have been commanded suddenly to flash into living tissues? Do they believe that at each supposed act of creation one individual or many were produced? Were all the infinitely numerous kinds of animals and plants created as eggs or seeds, or as full grown? And in the case of mammals, were they created bearing the false marks of nourishment from the mother's womb?' Darwin might well allow himself to ask these questions, for he and Wallace had found the answer to them.

So soundly was the theory of evolution by natural selection grounded that research does

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nothing but confirm the links in its chain of evidence and the inferences to be drawn from them. Its field has extended from the explanation of the production of plants and animals to every aspect of the intellectual life of man, and it would be imprudent to doubt that its greatest triumph may yet lie in the highest aspect of that life. Some persons have attempted to discredit natural selection, on the grounds that being a destructive agent it cannot produce anything new or make the fit fitter. Such persons have only demonstrated that they have neither understood the problem nor studied 'The Origin of Species', in which Darwin carefully pointed out that 'several writers have misapprehended or objected to the term Natural Selection. Some have even imagined that natural selection induces variability, whereas it implies only the preservation of such variations as arise and are beneficial to the being under its conditions of life.' Variation produces novelties at random, but selection determines which are preserved. Only a genius could have discovered a key of such simplicity to so great a problem. Only ignorance, neglect of truth, or prejudice could actuate those who, in the present state of knowledge, without discovering new facts in the laboratory or in the field, seek to impugn the scientific evidence for evolution.

With such new formulations as may be required, the concept of evolution by natural selection continues and will continue to provide what Darwin hoped when he wrote in 1837 in his Notebook: 'My theory would give zest to recent and fossil comparative anatomy; it would lead to the study of instincts, heredity and mind . . . to closest examination of hybridity—to what circumstances favour crossing and what prevents it—and generation, causes of change in order to know what we have come from and to what we tend. This and direct examination of direct passages of structure of species, might lead to laws of change, which would then be the main object of study, to guide our speculations.'

With the same confidence as it accepts Copernicus's demonstration of the movement of the Earth round the Sun and Newton's formulation of the laws of this movement, science can now celebrate the centenary of the first general principle to be discovered applicable to the entire realm of living beings.

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A progress report on titanium

N. P. INGLIS and M. K. McQUILLAN

The introduction of a new structural metal is so rare that the story of the research and development involved would in any circumstances be interesting. This is particularly so for titanium, which has changed from a laboratory exhibit to an industrially important metal within the space of about ten years. The technical problems of its extraction, melting, alloying, and processing have been very considerable, but they have been so far solved that world production of titanium has risen from 3 tons in 1948 to 25 000 tons in 1957.

As long ago as 1791 the existence of titanium was recognized by William Gregor, the English chemist and mineralogist, and three years later it was independently discovered by the German chemist Martin Klaproth. Not until 1925, however, was the metal produced in a form pure enough for its properties to be accurately assessed. Realization that titanium both has an exceptional combination of physical properties and is very widely distributed in nature—of the truly structural metals only aluminium, iron, and magnesium are more abundant—made the metal of great interest to engineers, especially those in the aeronautical field.

The present price of granular titanium is about 16s. per pound, and that of wrought forms ranges from about £2 10s. to £10 per pound, according to the amount of working. The relatively costly processes of extraction, melting, and fabrication make it unlikely that the metal will ever become as cheap as, say, aluminium, but while its wrought forms are now 6 to 10 times as expensive as stainless steel, allowing for the difference in weight, the ratio might well fall to 2 or 3 times if use and production were expanded considerably. These prices and ratios refer to wrought forms like plate, sheet, or bar. As the cost of fabricating these wrought forms into the article required is often appreciable, and is little or no greater for titanium than for stainless steel, the ratio of cost of finished article in titanium compared with steel might ultimately be appreciably less than 2 or 3 to 1.

Since the unusual properties of the metal have been the reason for the great technical effort necessary for its production on an industrial scale, it is appropriate to consider these first.

PROPERTIES

Briefly, the important properties of titanium are its relatively low specific gravity, its ability to form alloys of very high strength, and its considerable corrosion resistance. Before any new structural metal can be established, however, a wide range of its properties must be determined. The research done on the physical and mechanical properties of titanium and its alloys is too extensive even to be summarized here, and our account must be restricted to those properties which engineering designers usually regard as being of the first importance.

Chief among these is mechanical strength, which for engineering purposes must be allied with ductility. In titanium, strength and ductility are very sensitive to the presence of impurities, particularly oxygen, small quantities of which can both strengthen and embrittle the metal. In commercial titanium production the highest degree of purity (i.e. the lowest strength and greatest ductility) is the usual target, on the grounds that it is better to start with a material which is readily workable and to harden it as required. Current 'commercially pure' titanium has therefore a tensile strength of not more than 30 tons/sq. in. and very high ductility. The range of strengths which can be covered by titanium-base alloys, however, extends up to 80 tons/sq. in.

Under repeated loading, titanium has the advantage over most other non-ferrous metals of a definite fatigue limit, as exhibited by steels. Moreover, under complete reversals of stress the fatigue limit is 50–60 per cent of the ultimate stress, which is an exceptionally high ratio. In practice, however, the fatigue strength of any metal is likely to be less than that determined by standard laboratory tests because it is considerably reduced by any sudden changes in cross-section such as result from the notches and grooves found in most engineering parts. Consequently many tests have been carried out to determine the effect of standard notches on the fatigue strength of titanium and its alloys (see figures 1 and 2). For all materials notch-sensitivity is rather greater when tested under conditions of

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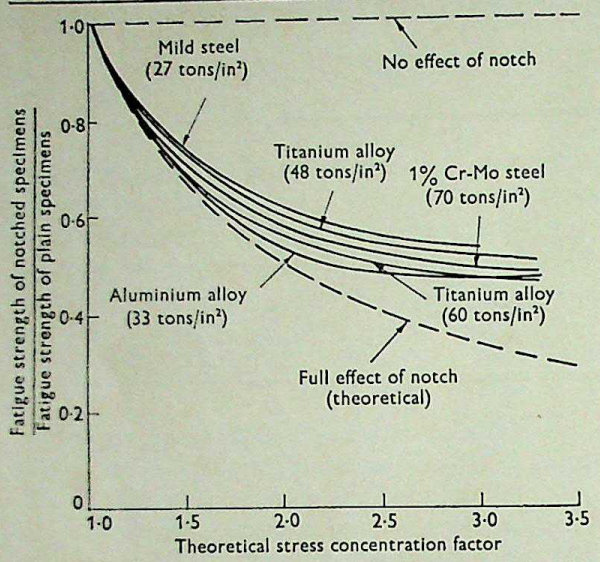

FIGURE 1 – *Notch fatigue sensitivity of various metals when subjected to repetitions of bending stress.*

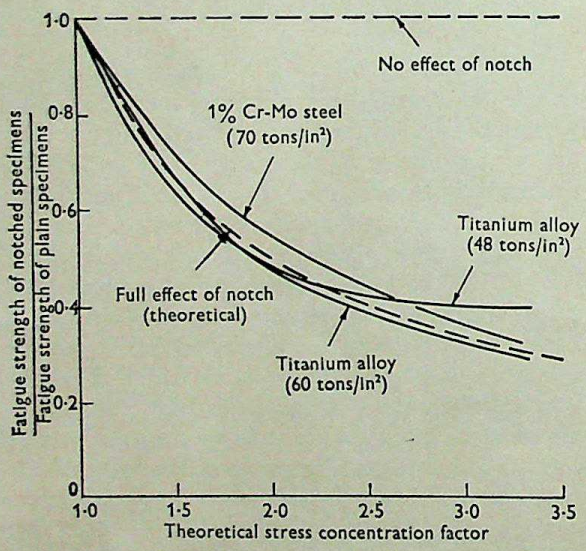

FIGURE 2 – *Notch fatigue sensitivity of various metals when subjected to repetitions of direct stress.*

reversed direct stress than under conditions of reversed bending stress. Under reversed bending stress the notch-sensitivity of titanium and its alloys is much the same as that of other constructional materials. Under reversed direct stress the stronger titanium alloys are rather more notch-sensitive than alloy steels in the same tensile range, but there is no reason to suppose that titanium is unduly sensitive to notch fatigue effects.

At one time it was hoped that titanium would have good load-carrying ability at really high temperatures, but in this respect it has proved disappointing. The strength falls fairly rapidly as the temperature rises, particularly above 400° C (figure 3). Certain alloys have better elevated-temperature strength; but none is outstanding, and the best so far tested had good load-carrying ability up to only 500° C.

Titanium has excellent resistance to corrosion. As with high-chromium steels and aluminium, this is due to the presence of a strong tenacious film of oxide which heals rapidly when damaged. Titanium is in fact resistant to a wider range of corrosive substances than are chromium-nickel austenitic steels. Its superiority to such materials is particularly marked in the case of solutions of chlorides, including sea-water. Even in polluted sea-water, titanium has proved to be unaffected after many thousands of hours under jet-impingement conditions and under conditions which normally give rise to differential aeration effects. Moreover, the corrosion fatigue strength of titanium is actually greater in sea-water than in air; for other corrosion-resistant metals the ratio of the two rarely exceeds 60 per cent.

The outstanding corrosion resistance of titanium has caused growing interest in its use in bone surgery, where its strength, ready availability in wrought form, and complete resistance to body fluids give it many advantages over alternative materials.

Like aluminium, titanium can be anodized in acids, such as sulphuric or phosphoric acids, to produce an even more protective layer of oxide, which may be up to 0·4 μ thick, depending on the applied voltage. The limiting thickness is that at which the electrical resistance of the film prevents further flow of current. Rather surprisingly, electrical contact with the underlying metal can still be made through the anodic film by applying light pressure to the surface—a characteristic which makes titanium useful for constructing commercial anodizing equipment. Anodic polarization can be used to great practical advantage to form self-healing films which allow titanium to be used even in some of the few corrosive media to which it is not normally resistant. This is done by raising the electrical potential of the titanium, either by direct connection to an external potential source, or sometimes more conveniently by coupling the titanium with a piece of more noble metal with which it will form an electrolytic cell when immersed in the corrosive medium. As the anodic film is built up the flow of current virtually ceases, but it is immediately available to repair the film if it should be broken.

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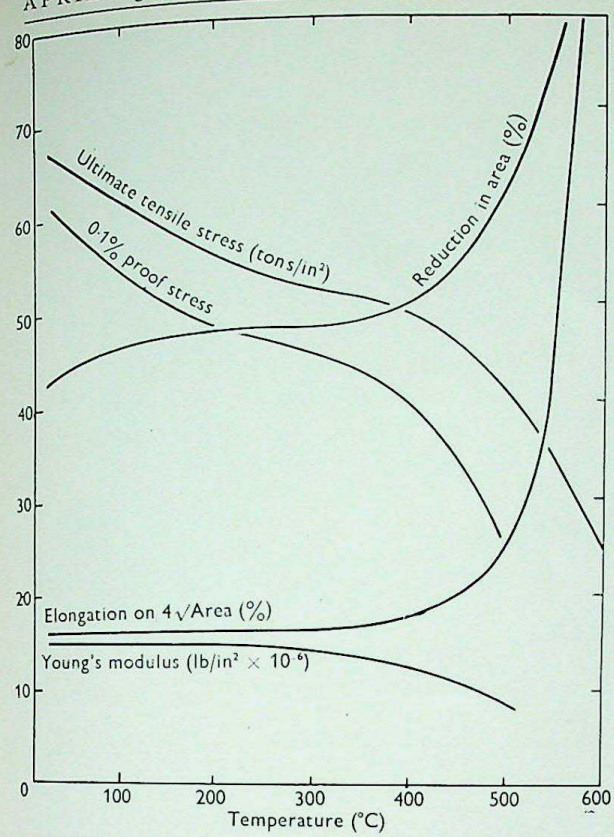

FIGURE 3 – *Effect of temperature on the tensile properties of a typical alpha-beta titanium alloy.*

TITANIUM ALLOYS

Unalloyed 'commercially pure' titanium has found many uses, especially in sheet form. In the very important aeronautical field, however, the most economically attractive uses for the metal demand high strength combined with low density. To achieve the high strengths necessary, alloys are essential.

The alloying behaviour of titanium is complicated by the fact that titanium is a transition metal, and like all such elements possesses an incompletely filled inner electron shell having an energy level very close to that of the outermost shell. It is usually assumed that in metallic titanium the two shells form a common band of possible electron levels, and that electrons from either can thus take part in bonding processes and electrical conduction. It is not certain, however, that all the electrons in the incompletely filled shell must necessarily contribute to the common band, and much more experimental evidence will be necessary before a truer picture of the electronic structure of titanium can be formulated.

Although existing theories can do little to help in practical alloy development, the behaviour of

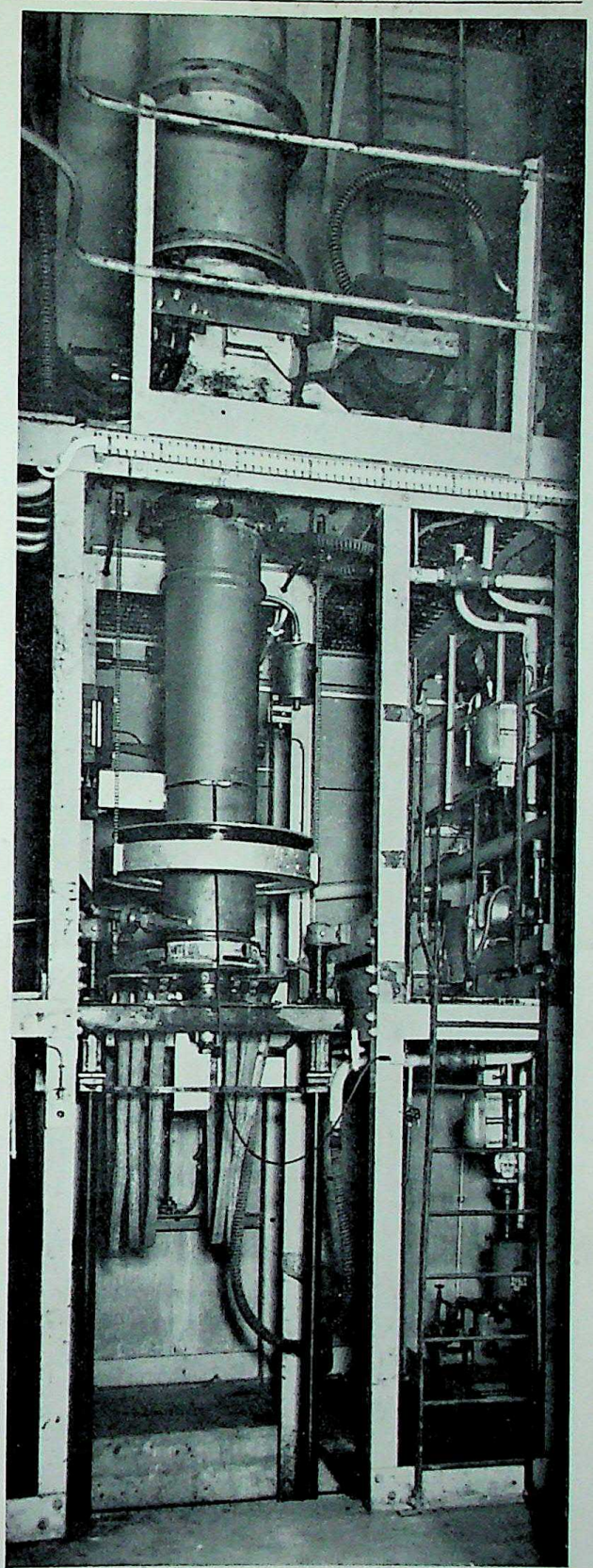

FIGURE 4 – *Production unit for vacuum melting of titanium, housed in blast-proof concrete enclosure.*

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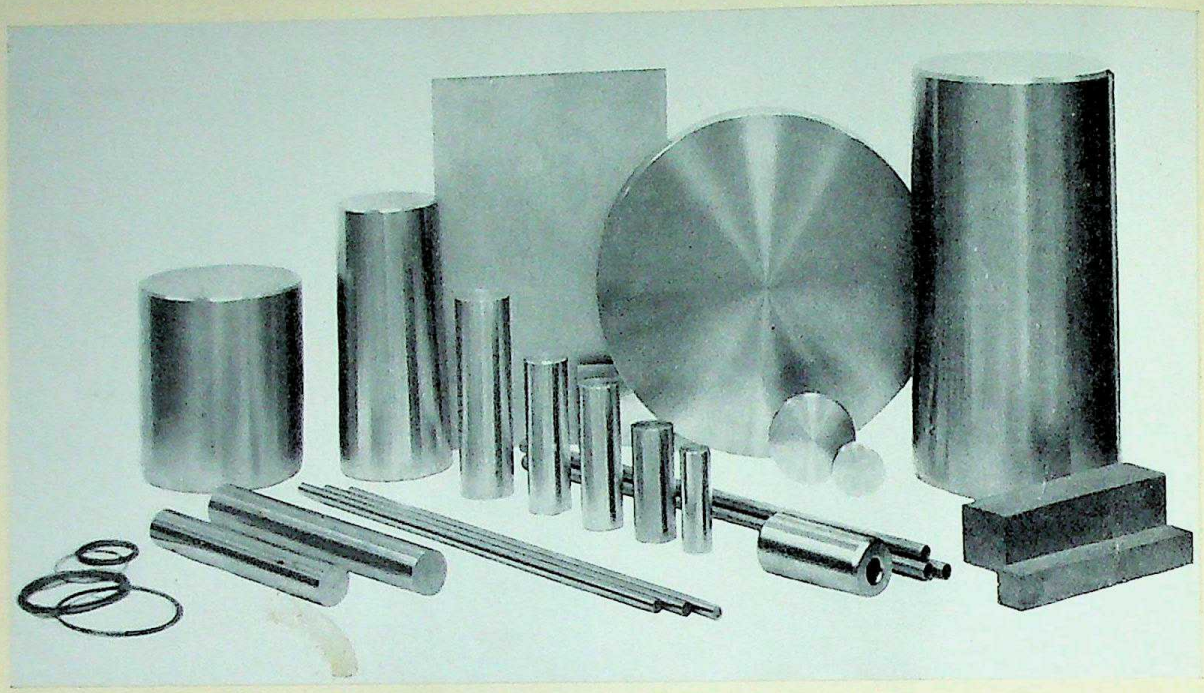

FIGURE 5 – *Examples of wrought forms in titanium and titanium alloys.*

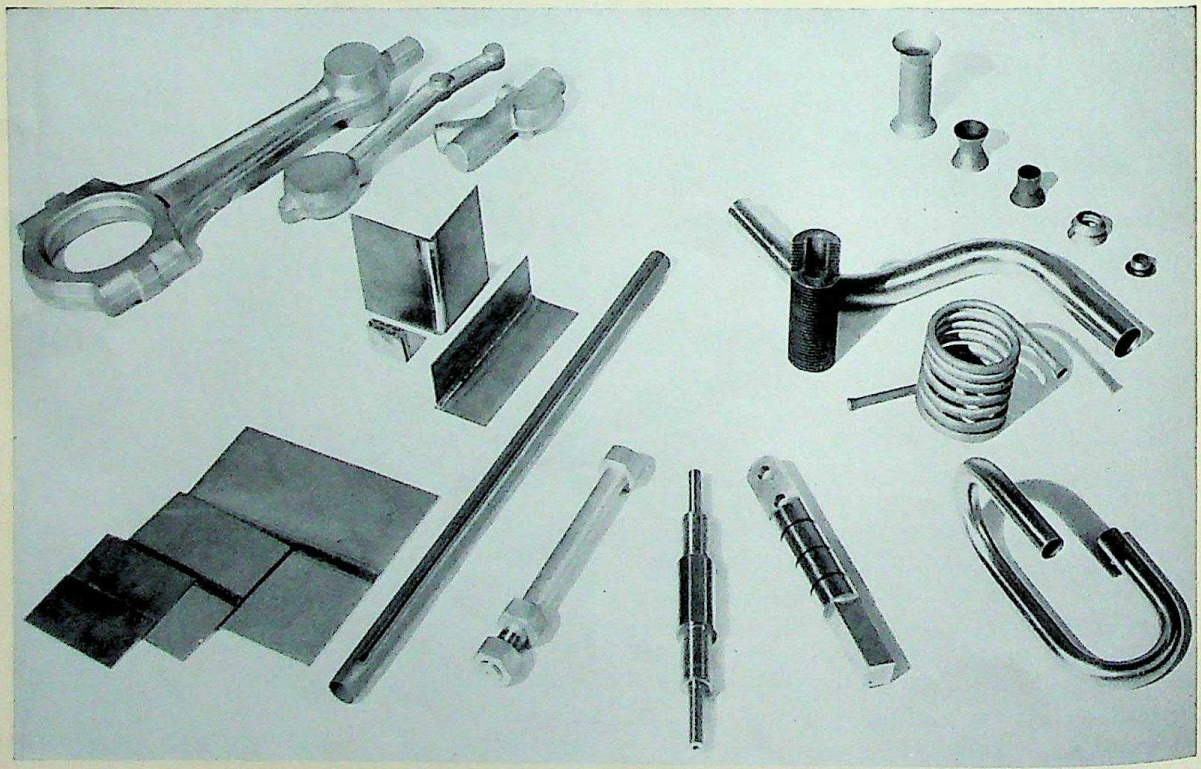

FIGURE 6 – *Examples of forged, welded, machined, and manipulated titanium.*

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titanium alloys can be empirically systematized to a useful extent. Broadly speaking, for example, alloys formed between titanium and other transition elements have considerable similarities among themselves and differ in important respects from alloys formed with non-transition elements. This is true in spite of the variation within the two groups of factors—such as atomic size, valency, and electrochemical properties—that are usually important in determining the behaviour of alloys.

Titanium alloys are usually classified in terms of crystal structure. Up to 882·5° C titanium has a hexagonal structure and is designated alpha-titanium; above this it adopts a body-centred cubic form known as beta-titanium. Addition of other elements can stabilize one of these structures at the expense of the other, and by appropriate alloying and heat treatment it is possible to obtain materials that consist entirely of the alpha phase, entirely of the beta phase, or of a mixture of the two. These constitute the principal types of alloy.

Alpha, beta, and alpha-beta alloys each have characteristic mechanical properties. To a large extent this is directly attributable to the different atom-packing arrangements in the two phases, but there may also be an effect due to differences in the electronic interaction between titanium atoms and those of alpha-stabilizing and beta-stabilizing elements.

Alpha alloys are strong, but are difficult to fabricate, especially into sheet. Beta alloys, by contrast, are easy to work but are normally appreciably less strong, and may be unstable if used at elevated temperatures. They have the advantage over alpha alloys that their strength can be made high by heat treatment; even then, however, they tend to have a lower resistance to creep (i.e. to slow deformation under prolonged load at elevated temperatures) than alpha alloys that are of similar strength at ordinary temperatures.

The alpha-beta alloys are intermediate in type. When heat treated, they can combine high strength with rather better ductility than can usually be obtained in other alloys of comparable strength. Most of them contain an element, usually aluminium, which dissolves in and strengthens the alpha phase, together with one or more of the elements that stabilize the beta phase. The latter are usually chosen from among the early transition metals such as manganese, vanadium, niobium, tantalum, and molybdenum. For some time an alloy containing 4 per cent aluminium and 4 per cent manganese has been the most widely used alloy of this class, but there is now a tendency to use new compositions such as 6 per cent aluminium with 4 per cent vanadium and 6–7 per cent aluminium with 3 per cent molybdenum.

The range of elements that can be added to alpha alloys is limited by the very low solubility of many metals in alpha-titanium. So far only aluminium and tin have been used commercially as major additives. Zirconium is another possible major additive, and there are a number of possible minor additives which may give improved properties if their tendency to make the metal brittle and other difficulties can be avoided.

No beta alloys have yet been exploited commercially, principally because they tend to become brittle at elevated temperatures unless a very large proportion of a beta-stabilizing element is added. Nevertheless, beta-type alloys that can be heat treated may eventually be used. Stable beta alloys are also potentially useful in applications demanding high corrosion resistance. A titanium alloy containing 30 per cent of molybdenum, for example, is very promising for use in the construction of chemical plant.

At the present time much attention is being devoted to the development of an alloy for use in sheet form. This must be soft enough for easy fabrication but capable of being considerably strengthened by heat treatment. Several such alloys have been proposed, all containing fairly large amounts of beta-stabilizing elements. In the soft condition they usually contain both alpha and beta phases, the beta being subsequently hardened by heat treatment at a low temperature. Such alloys are now becoming commercially available.

Alpha, beta, and alpha-beta alloys have now been fairly thoroughly explored. Most of the advantages resulting from the purer raw materials made possible by modern methods of extraction have already been realized. Future developments may be expected to include the appearance of new classes of alloys, such as one consisting of a hard intermetallic compound finely dispersed in an alpha matrix. An alloy of this type might be produced by a classical age-hardening process or by tempering the martensitic alpha phase which in a wide range of alloys is formed from the beta phase by a shear process on quenching.

Major improvements, however, will depend on a more fundamental understanding of the metallurgical conditions which limit the extent to which some alloys can be worked and which make others become brittle in use. It can fairly be said that progress is being made in this direction.

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EXTRACTION

The need to eliminate as much oxygen as possible determines the type of process by which titanium is extracted from its ores. As it is impossible to reduce the oxygen content to an acceptably low level by direct reduction of the oxide, commercial production is in two stages.

The ore at present used is rutile (TiO_2), although it must eventually be the more plentiful ilmenite ($FeO.TiO_2$). This is first converted to titanium tetrachloride, which after careful purification is reduced by either magnesium or sodium. Magnesium reduction, first applied by W. J. Kroll, has been widely adopted in the United States and Japan. British production, however, depends mainly on sodium reduction, and the most recently constructed plant in the United States also uses this method. The two processes yield metal of much the same purity and at much the same price. Of the two, the sodium reduction method probably has the greater potential for improvement, both economically and in the purity of the product,

As has been stressed, the strength of titanium is very sensitive to the presence of traces of oxygen. Since the strength of a metal is, in general, proportional to its hardness, the hardness of a small melted ingot is generally accepted as a convenient index of purity of the raw metal. Changes in the average hardness of commercial titanium indicate the improvements that have been made in recent years. Very pure titanium, made by thermal decomposition of the tetraiodide, rarely has a hardness lower than 80 D.P.N.[1] The average hardness of titanium now produced in Britain by the sodium reduction process—believed capable of ultimately yielding a still purer product—is 130 D.P.N. Only a few years ago the hardness of commercial titanium was in the range of 160–200 D.P.N.

Much attention has been, and is being, given to electrolytic processes for the extraction of titanium. A short time ago many people were confident that the history of aluminium would be repeated and that chemical reduction would be replaced by electrolysis. Lately, however, confidence in the commercial future of electrolytic methods has waned considerably. This is not due to fundamental difficulties in producing titanium by electrolysis but because such processes seem likely to be more costly than those now used.

Recent work on electrolytic purification of scrap or impure titanium suggests the possibility of a two-stage process whereby very crude metal is first produced and this is then refined electrolytically, but preliminary estimates of cost are not favourable, particularly in the light of proposed economies in existing processes.

MELTING

The extreme reactivity of molten titanium, which no refractory resists, necessitates methods of melting radically different from those used for most other metals. It is now fairly general commercial practice to use direct-current arc melting in a water-cooled copper crucible and to protect the molten material from atmospheric contamination by working in argon or *in vacuo* (figures 4 and 7).

Formerly a water-cooled tungsten or carbon electrode was employed, the titanium being fed into the crucible in the form of granules or pellets. In this method difficulties were encountered, however, through impurities being picked up and the ingot not being homogeneous. Although a furnace with a non-consumable carbon electrode was developed to a type which reduced carbon contamination to less than 0·05 per cent, it became abundantly clear that a consumable titanium electrode would have the following overriding advantages:

1. Elimination of carbon (or tungsten) contamination. Even at the 0·05 per cent level this could be detrimental in certain alloys, especially if segregation occurred.
2. Consumable-electrode furnaces are more readily adapted for operation *in vacuo*.
3. Higher melting rate for a given power input.
4. Avoidance of the not inconsiderable engineering difficulties of maintaining a uniform separate feed of titanium and alloying elements.

Consumable electrodes for feeding into the furnaces are made by mixing raw titanium granules with the desired alloying elements and compressing the mixture into pieces strong enough to be handled, pressures of 10–20 tons/in^2 being required. These pieces are welded together until they correspond to the weight of the ingot to be produced, which may be up to 2000 lb in Britain and frequently appreciably more in the United States. It may be remarked that the granular raw

[1] Some confusion can result from the use of more than one scale of hardness. In Britain it is usual to use the diamond pyramid number (D.P.N.) rather than the standard Brinell hardness number. Within the range with which we are here concerned the standard Brinell hardness number is about ten units less than the D.P.N. The sub-standard Brinell test favoured by United States suppliers gives a hardness number ten units lower still, i.e. some twenty units less than D.P.N.

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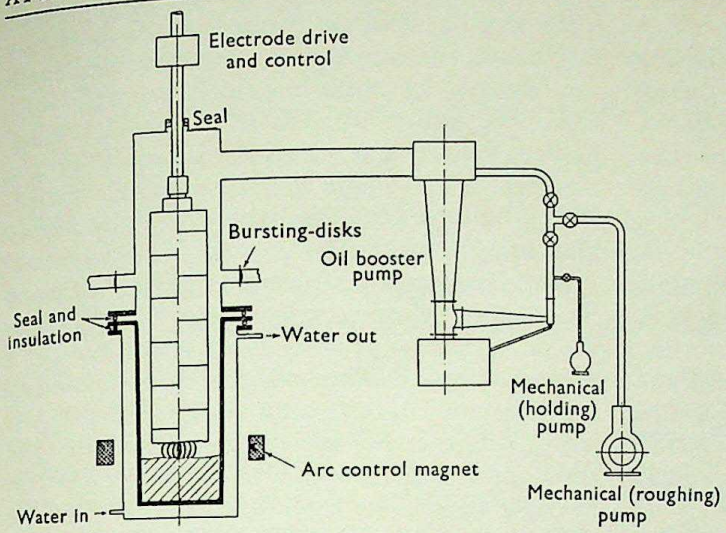

FIGURE 7 – *Diagrammatic representation of vacuum melting furnace with consumable electrode.*

titanium produced by sodium reduction is very much more easily compacted than the coarser product of the Kroll process.

The argon atmosphere used in furnaces operating with non-consumable electrodes is now usually replaced by a vacuum when consumable electrodes are used, and even the largest furnaces are being operated at pressures as low as 5–20 μ of mercury. The operating pressure has to be modified, however, when the alloy to be melted contains relatively volatile elements such as manganese. Double melting, in which the ingot made in the first melting becomes the consumable electrode in the second, has become fairly general. This improves homogeneity, gives a better surface to the ingot, and reduces the hydrogen content.

Arc melting in water-cooled crucibles has the disadvantage of being potentially dangerous. If the wall of the crucible should become perforated, water will come into contact with titanium at about 1700° C. The resulting generation of steam may cause an explosion, and this may be followed by a hydrogen-air explosion if the crucible should burst. Great precautions are taken to avoid such events, and to prevent their being dangerous if they do occur. Bursting-disks are fitted (figure 7) to prevent the build-up of steam pressure. Furnaces are situated behind thick concrete walls and operated by remote control (figure 4).

The intensive development work done on melting methods for titanium is likely to prove extremely valuable for some other of the newer metals, such as zirconium, in which there is a growing interest. Melting in a vacuum is also of interest for some of the older metals, the properties of which are often appreciably modified by the vacuum-melting process.

FABRICATION

As might be expected, the conversion of titanium ingots to plate, bar, and other wrought forms has generally been carried out with equipment used for similar processes on other metals. Despite the relatively high melting point of titanium, no mechanical difficulties have been encountered in forging or hot rolling. Cold rolling of 'commercially pure' titanium is also straightforward and calls for no comment, though this is not true for alloys.

Hot working

The difficulties experienced here are chemical rather than mechanical. They are concerned with the prevention of undue contamination by oxygen, nitrogen, or hydrogen, with which titanium reacts readily at the temperatures involved. Fortunately, diffusion of oxygen and nitrogen in titanium is relatively slow, and by careful heating procedures these impurities can be confined to a thin surface layer which can be removed by mechanical or chemical methods. Hydrogen, on the other hand, diffuses very readily, and although it can be removed by heating *in vacuo*, the more usual course is to prevent trouble arising by strict control of the furnace atmosphere.

Hot extrusion has not proved as successful for titanium as for many other metals. This is because when forced through a die titanium is prone to seize, causing imperfections in the metal surface. Much attention is being given to this problem, the solution of which clearly depends upon effective high-temperature lubrication. Some success has been achieved by sheathing the workpiece in copper and by the use of glass as a lubricant, but the latter has so far proved less satisfactory for titanium than for other metals.

Cold working

The cold rolling of alloys of high tensile strength is possible only by repeated small reductions, with frequent intermediate annealing. The difficulty is not specific to titanium: it is encountered in the cold rolling of any metal with a tensile strength of 60–70 tons/in^2 and a high ratio of yield stress to ultimate stress. In the case of titanium, however, the difficulty is so great that, with equipment now available in the metal industry, thin sheet (of the

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order of 0·05 cm thickness) in the harder alloys has so far been almost entirely produced by hot rolling. It is usual to pack a number of thin sheets together and roll them within steel cover-plates. Such a method gives, however, poorer surface finish and dimensional tolerance than if finishing were done by cold rolling, and ways of overcoming the difficulty are being studied.

The problem can be approached in two main ways, both of which are being closely investigated. The metallurgist's approach is to find an alloy that is soft enough to be rolled easily when cold and can then be strengthened by heat treatment. The engineer can seek a solution by studying the effect of surface treatment and lubrication on the rolling loads required: for example, it has been shown in some very recent experiments that a newly developed oil can reduce by 50 per cent the load normally required to achieve a given reduction in thickness.

As in extrusion, cold drawing is hampered by titanium's tendency to seize, but various methods of reducing this difficulty are available. In fine wire drawing, for example, silver-plating of the stock has proved a very satisfactory method of lubrication and one which remains effective even when drawing is continued after annealing. Various chemical treatments—such as treating with cyanide or phosphate or controlled oxidation at a high temperature—are also useful in providing a surface which will hold a lubricant and prevent direct contact between the metal and the die.

Welding

Titanium may be welded by most of the normal methods provided that certain specific precautions are observed, such as prevention of contamination by oxygen, nitrogen, and hydrogen.

In fusion welding, only arc processes carried out in an inert atmosphere can provide sufficient protection for the heated metal. Tungsten arc welding, shielded by inert gas, is usually adopted, but special baffles have been designed to improve the gas shielding, together with a water-cooling system to chill the heated metal as quickly as possible. In appropriate circumstances fusion welding can be done in a chamber filled with inert gas, which obviates the need for either baffles or cooling. Pressure welding is possible at temperatures of about 800° C provided the surfaces to be joined have been very carefully cleaned and abraded.

The welding of ordinary commercial titanium and of alpha alloys need have no harmful effect on the mechanical properties of the metal, but this is not true of alpha-beta alloys. Welded joints in such alloys are brittle, even where contamination is negligible, owing to reactions which occur in them on cooling. Heat treatment can improve the ductility of samples made brittle in welding, but does not usually fully restore the original properties.

CONCLUSION

Although the technological development of titanium has been considerable over the past ten years, much remains to be done before the metal can be exploited to its fullest advantage. In particular, the main objective of future research must be a reduction in costs. Many factors could contribute to such a reduction, but the principal ones are likely to be:

1. Increased production, resulting from more extensive use.
2. Improved methods of fabrication, resulting in the final article containing a much higher percentage than at present of the original titanium used.
3. Specific utilization of the unique combination of properties of the metal instead of, as at present, using it merely as a substitute for other metals.

These factors, it is suggested, are at present more important than improvements in the method of reduction and improvements in alloys, although progress in both fields would be of great value.

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Rockets and satellites in scientific research

H. S. W. MASSEY

Any day may bring the unheralded announcement of an important new achievement in the field of rockets and satellites. To review progress is difficult, but because of its many far-reaching implications it is important to see the field as a whole. Professor Massey considers here both what has been achieved and the likely course of future developments. He stresses that satellites and vertical sounding rockets are not competitive but complementary.

Scientific study of the Earth's atmosphere and of the surrounding interplanetary space has been proceeding for a long time, but until 1945 it was possible to take instruments only up to altitudes of eighteen miles or so, in exploratory balloons, in order to make direct measurements of atmospheric properties. There is much of interest and importance in the atmosphere at much higher altitudes —the ionosphere really begins only about 60 miles up and extends up to several hundred miles; auroral displays occur mainly at heights above 60 miles; the atmospheric currents responsible for the quiet-day magnetic variations are strongest between 60 and 90 miles; meteors rarely penetrate closer than 60 miles from the surface before being evaporated; and so on. We now possess a considerable store of knowledge of these upper atmospheric phenomena; but much more remains to be done, and the sounding rocket is becoming of increasing importance in this direction. A rocket carries with it not only its fuel, but also its own oxidant, so that as far as its propulsion is concerned, it is independent of its surroundings.

In view of the fact that even today most of our knowledge of upper atmospheric phenomena has been derived from observations using ground-based radio or sonic probing, spectroscopic studies, magnetic measurements, and so on, it may well be asked why it is necessary to use sounding rockets at all. The answer is that although they involve difficult and expensive techniques, for some purposes their use is essential.

The most important of these is the study of the solar radiation before it enters the absorbing atmosphere. It is the interaction of the solar electromagnetic radiation with the atmosphere that produces the ionosphere and the night airglow, and affects the chemical constitution of the atmosphere (above 60 miles the oxygen is largely atomic). Only radiation that has not interacted reaches the ground, and therefore its observation provides no useful information about the primary origin of the solar-dependent phenomena. To observe the radiation that is effective in the atmosphere, it is necessary to send measuring equipment up to altitudes above the regions in which the interaction is strong. The sounding rocket is the only vehicle that can carry equipment to the altitudes involved.

Apart from this most important function, there are many others that cannot be fulfilled by indirect ground-based studies. These include measurement of the variation of electron concentration in the ionosphere between the main layers and above the uppermost (F_2 layer) maximum; the nature of the ions at different heights in the ionosphere; the variation with height of the intensities of the different lines in the night airglow; the relative concentration of atomic and molecular oxygen at high altitudes; and others.

Another possibility which is opened up is that of experimenting with the upper atmosphere. At any desired altitude the local concentration of a particular constituent may be greatly increased by ejection of this material from a rocket. The resulting effects may be observed with ground-based instruments. Experiments of this kind have been carried out with very interesting results, using sodium and also nitric oxide as the ejected material (see page 88).

Apart from these unique facilities, it is also an advantage to be able to make measurements directly instead of deriving them indirectly. It is usually possible to obtain more detailed knowledge of the variation with height of a particular quantity. It must always be remembered, however, that the difficulty of making the measurements from a rocket is often so considerable that undue emphasis should not be placed on data obtained in this way until the techniques involved have been thoroughly tested and, as far as possible, checked with other methods.

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THE NEED FOR ARTIFICIAL SATELLITES

Many important upper atmospheric phenomena not only vary markedly with latitude and longitude, but are variable in time, and the great limitation of the sounding rocket is the short time of flight—a few minutes at most. The sun is a variable star, and the changes which occur in its radiation produce correlated effects in the atmosphere. During disturbed periods the solar electromagnetic radiation may change appreciably, but in addition streams of charged particles may be emitted. These produce effects on the Earth that depend very much on the latitude, because the Earth's magnetic field directs the charged particles to the auroral zones. The latter are belts usually about 5° wide, located about 23° from either pole. Among these effects are magnetic and ionospheric storms (including polar black-outs) and auroral displays. Radio fade-outs are also due to solar activity, but they result from increased emission of ultra-violet or X-radiation during solar flares.

It is clear that for the study of these important aspects of atmospheric phenomena, sounding rockets which travel rapidly over a nearly vertical trajectory are very inadequate. For this reason it is appropriate to consider what can be achieved with artificial satellites. These also require the use of high-altitude rockets, but in this case to take an instrument container up to a suitable altitude and to launch it there in the right direction and with the speed necessary for it to circulate as a satellite. If the lifetime of such a satellite is long enough and the data obtained by its measuring instruments can be transmitted to the ground in the form of coded radio signals, chosen atmospheric properties, or the solar radiation itself, can be studied continuously not only as a function of time, but also over the whole, or a large part, of the Earth's surface. The problems involved in doing this will be described briefly later in this review. It is quite clear from the progress already made that such satellites will be a permanent feature of future atmospheric, solar, cosmic ray, and space research.

It is of interest to point out here that precision observation of the orbit of a satellite circulating outside the effective atmosphere will make possible the more exact determination of the figure and density distribution of the Earth. On the other hand, if the orbit passes through the high atmosphere it is possible, from observations of the orbit, to obtain information about the density of this part of the atmosphere.

The introduction of artificial satellites does not mean the supersession of the vertical sounding rocket. If it is to have a useful life, a satellite must circulate at altitudes above 120 miles, so that variations of properties in the vertical direction below this altitude must still be measured by rocket sounding. The two techniques are complementary rather than competitive.

SOUNDING ROCKETS

The first rockets to be used for atmospheric research were captured German V-2 military rockets. These had a gross launching weight of about 12 tons, and had a total length of 46 feet and a maximum diameter of 65 in. They employed liquid propellents making up over two-thirds of the weight at launching; the fuel was alcohol and the oxidant liquid oxygen. An instrument load of 2200 pounds could be carried in these rockets up to a height of 100 miles. The first upper atmospheric observations made with rocket-borne instruments were carried out in the United States in 1946 using V-2 rockets. Over 60 instrumental flights have been made with them.

Early in 1949 a large rocket, the Viking, designed and developed in the United States, was used for atmospheric sounding. It will transport an instrument load of 750 pounds to a height of over 130 miles and attain a maximum velocity of about 5400 feet per second. Like the V-2, it is stabilized, while the motor is burning, against roll, pitch, and yaw by a control system; in addition, the later versions are fitted with controls that are effective after burn-out. The Viking is too expensive for normal vertical-sounding purposes, but is important in the American Vanguard programme (page 89); the first stage of the launching rocket is essentially a Viking rocket.

The Aerobee rocket was the first designed in the United States purely for upper atmospheric sounding. It is about 20 feet long, has a gross weight at launching of 1068 pounds, and is capable of carrying a payload of 150 pounds to an altitude of 60 miles. Recently an improved version, known as the Aerobee-Hi, has been developed, and this will carry equipment to a little less than twice the height. A research rocket with liquid propellent similar to the Aerobee, has been developed in France for atmospheric sounding. It is known as Véronique.

Another example of a sounding rocket using a liquid propellent is the Russian meteorological rocket. This uses nitric acid as oxidant and alcohol as fuel; it attains an altitude of about 60 miles. No information is available at the time of writing

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about the Russian geophysical rocket, which can attain much greater altitudes.

The first single-stage sounding rocket using solid fuel is the Skylark, designed in Britain at the Royal Aircraft Establishment. This is being employed in the British Upper Atmospheric Research Programme. It is capable of carrying a payload of 150 pounds to altitudes of 120 miles. At launching it weighs over 1 ton.

Much smaller rockets may be employed if multi-stage or other combined systems are used. Thus a balloon may be used to carry a rocket up to an altitude of 80 000 feet, well above the dense atmosphere, the resistance of which very substantially increases the propellent required for a ground-launched rocket. By firing from 80 000 feet the same minimum altitude may be achieved with a much smaller rocket. This 'rockoon' combination has been used very effectively by American scientists. As an elaborate launcher is not required, firings may be carried out in different locations, the only extra complication being the safety requirements—it is not easy to predict where a balloon-launched rocket will eventually come back to earth. Very recently a very high altitude, between 1000 and 4000 miles, was attained in American experimental firings at Eniwetok by exploiting the rockoon technique to the full, with a large balloon and a large rocket.

The Australian and Japanese high-altitude rocket projects both use the rockoon principle.

Two-stage rockets that are in use in the American programme are the so-called Nike-Deacon and Nike-Cajun combinations. In these, the Nike rocket motor is used to take the small Deacon or Cajun rockets quickly up to altitudes comparable with that reached by the balloon in a rockoon.

Development of sounding rockets that are still more economical is proceeding steadily, particularly in the United States and Japan. Thus in the United States, van Allen has used a rocket only 3 inches in diameter which when launched from a balloon, reaches a height of 65 miles.

TECHNIQUE OF MEASUREMENT WITH ROCKET-BORNE INSTRUMENTS

It is not possible within the space of this article to describe the various techniques employed in making measurements with instruments carried aloft in rockets. The chief difficulties are the limitations of space and weight; the shocks and vibration to which the instruments are subjected; the tendency of uncontrolled rockets to pitch, yaw, and roll; the short flight-time; the problem of recovering information; and the modification of the local atmosphere produced by the rocket itself.

The last of these is often a serious complication For example, it is impossible to measure ambient pressures less than 10^{-5} mm Hg with rocket-borne pressure gauges, because the pressure of gas given off from the rocket itself exceeds this. The Russian meteorological rocket avoids this kind of difficulty by ejecting the measuring instrument in a container from the rocket at a suitable altitude. This container is equipped with a parachute; it at first rises for some time and then falls comparatively slowly. Measurements are made only after ejection from the rocket.

Although several techniques for recovery of the instrument chamber from a rocket have been devised and operated, it is usual to arrange for the measurements to be converted to electrical impulses in a radio transmitter in the rocket and transmitted to ground as coded signals during flight.

ACHIEVEMENTS AND PROSPECTS OF RESEARCH WITH SOUNDING ROCKETS

Up to the time of writing almost all the published results of research using high-altitude rockets have been those obtained under the aegis of the United States Rocket Panel. This work dates back to 1946. It is not certain when the Russian research programme was initiated. Results of atmospheric structure measurements using the Russian meteorological rocket (see above and page 88) have been published during the past year, and it is likely that more results of Russian investigations will be published in the fairly near future. The British programme, which involves the firing at the Woomera rocket range in Australia, of Skylark rockets fitted with suitable instruments, is just coming into full operation. The French, Australian, and Japanese programmes are also likely to come into full operation during the International Geophysical Year.

Most of the American launchings have taken place at White Sands, New Mexico, but there have been firings from ships at sea. As a special contribution to the I.G.Y., a firing range has been established with Canadian co-operation at Fort Churchill in Canada, which is in the specially interesting auroral zone. An extensive series of rockoon firings over a latitude range from California to the Antarctic is also being carried out. The Russian I.G.Y. programme includes 100 firings of meteorological rockets in Franz-Josef Land and the Antarctic, as well as many launchings of higher-altitude rockets.

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As far as results are concerned, we must perforce confine ourselves almost exclusively to the American work, which is however both extensive and fruitful. The investigations carried out concern the solar radiation; the pressure, density, temperature, and wind distribution; the electron concentration and the composition of the ionosphere; the intensity distribution of radiation in the night airglow; the cosmic radiation; the flux of meteors and meteorites; the ozone and atomic oxygen content; the radiation present in the atmosphere above the auroral zone; and other features.

In the short space available here it is possible to single out only one or two specially interesting results. The nature of the incident solar radiation, although understandable in a general sense, has turned out to be rather different from that assumed earlier. In the visible region the sunlight is of an intensity and colour distribution not very different from that radiated by a black body at 6000° K. At shorter wavelengths the intensity falls well below this; it is, for example, only about one-fortieth for ultra-violet light at 2000 Å. On the other hand, in the X-ray region at wavelengths of 1–50 Å, the intensity is very much greater than would be expected for a black body at 6000° K. These X-rays probably come from the solar corona, the outer atmosphere of the sun, which is known to have a kinetic temperature of 1 000 000° K. Much interesting work is still to be done in the wavelength range between 50 and 1200 Å and also on the variability of the radiation at all wavelengths.

The observations of pressure and temperature made up to 50 miles using the Russian meteorological rocket agree quite well with those obtained by the Americans. There still seem, however, to be discrepancies at higher altitudes, indicating that the accuracy obtained at these altitudes still leaves much to be desired. For checking purposes alone the advantage of having many more rocket programmes in operation is clear.

One other remarkable feature of the American work has been the successful introduction of upper atmospheric experiments. In 1954, 5 pounds of sodium vapour was ejected at twilight from an Aerobee rocket at an altitude of 40 miles. It gave rise to a brilliant yellow fluorescence, due to irradiation by sunlight. This was visible with the naked eye for over twenty minutes, even at points 300 miles away from the launching site. There is normally only about 1 ton of sodium in the high atmosphere, but it contributes to the night airglow and exhibits a fluorescence at twilight that was intensified locally in this remarkable experiment.

In other experiments nitric oxide gas was ejected from rockets. During the day the nitric oxide cloud, ejected below 60 miles, is ionized by the sunlight and may be traced by radar reflection —there are possibilities here of artificially improving conditions for signalling. At night the gas was ejected at a higher altitude, where the oxygen is mainly atomic. Due to the reactions

$$NO + O \rightarrow NO_2 \quad \text{and} \quad NO_2 + O \rightarrow NO + O_2$$

which involve the catalytic recombination of oxygen, a great deal of radiation was emitted. Consequently the ejection produced a glowing cloud providing visual demonstration of the atomic character of some of the oxygen at the level concerned.

Scientific observation by ground equipment may be made on these artificially produced glows in the sky, and this can lead to very valuable information about the atmosphere. It may even provide new photochemical data from an environment in which complications due to surface effects are absent.

THE FIRST SATELLITES

In order that a body of mass m should circulate as a satellite round the Earth in a circular orbit of radius R measured from the centre of the Earth, it must move at a speed V given by

$$\frac{mV^2}{R} = \frac{GmM}{R^2}$$

where M is the mass of the Earth and G the constant of gravitation in the orbit. This equation merely expresses the condition that the pull of the Earth's gravity must be balanced by the centrifugal force. If r is the radius of the Earth,

$$\frac{GM}{r^2} = g,$$

the acceleration due to gravity at the surface of the Earth. Hence

$$V = \sqrt{(gr^2/R)}$$

If the orbit is not too far away from the Earth so that $r \simeq R$, we find that

$$V = 18\,000 \text{ miles per hour.}$$

In order to give the satellite a sufficiently long life to be useful, the initial orbit, which will in fact be elliptical rather than circular, should not approach too close to the surface. Otherwise the satellite will enter dense atmosphere at high speed and be disintegrated or evaporated by the resultant heating. This minimum distance of approach can be placed at about 100 miles or so.

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The practical problem is one of transporting a satellite, of sufficient size and weight to be useful for tracking and as an instrument container, to a height greater than 100 miles and then propelling it in a direction close to horizontal with a speed of 18 000 miles per hour or greater. This is a difficult control problem. The more reserve speed available the less accurate need be the direction of launching.

In the American Vanguard programme the satellite as planned weighs 20 pounds and is about 20 inches in diameter. The launching operation employs a three-stage rocket with a total weight of 11 tons. The first and heaviest stage is essentially a Viking motor, and the control mechanism is housed in the second stage. The first Russian satellite weighed 184 pounds. The second, in which the third-stage rocket and the satellite were not separated, was no less than six times heavier. There is at the time of writing no information available about the launching techniques employed, but it seems certain that much larger first-stage rocket motors were used than in the first satellite.

The first two Russian satellites both included radio beacons transmitting signals at 20 and at 40 Mc/s for periods of three weeks and one week respectively. These limited lives were probably due to battery failure, as the acceptable battery weight was restricted. In future satellites, power will be supplied from solar batteries, which will have very long life.

The first Russian satellite contained no equipment apart from the radio transmitters, but much interesting information has been obtained from its observation as well as that of the third-stage rocket case which accompanied it. In Britain, many universities and government departments recorded the radio signals. They have observed the Doppler frequency shifts due to the speed of the satellite in the line of sight and have used radio interferometers for direction finding. Visual, and particularly radar, methods of tracking have also been used with success. The frequencies chosen by the Russians are markedly affected by the ionosphere, so that the observed fading and refraction of the radio signals will provide much new information of importance about the upper ionosphere. The analysis of data obtained is still in progress.

The effect of air resistance on the first Russian satellite and rocket case was very marked, and both objects have now spiralled into the dense atmosphere and disintegrated. The minimum distance of approach in initial orbit was about 120 miles and its maximum about 580 miles. Early estimates of air density based on the number of revolutions performed before disintegration are consistent with estimates made by atmospheric physicists.

The second Russian satellite contained, in addition to the radio beacon, equipment for studying cosmic and solar radiation; a pressurized cabin with a dog; and additional radio for the transmission of observed data back to the ground. The dog's cabin included a store of food, a system of air conditioning, and apparatus for recording pulse and breathing rate, blood pressure, and electrocardiograms. No details are yet available about the results of the measurements, but it appears that a considerable amount of data was obtained.

The first American satellite, Explorer, launched by the Jupiter C research rocket, was cylindrical, with a length of 80 inches and diameter 6 inches, and weighed 30·8 pounds. The instrumentation included cosmic ray equipment, gauges for measuring internal and skin temperatures, and a microphone and erosion gauges for measuring the impact of micrometeorites. The radio beacons operated at 108 Mc/s, since radiation at this frequency suffers substantially no refraction in passage through the ionosphere and hence may be used to give accurate orbit data. Plans have also been announced for a satellite containing a transmitting television camera.

We can expect, in future satellites, more and more elaborate experimental equipment. The American I.G.Y. programme also includes magnetic field, and solar and earth radiation studies. Much can be done even by those countries who are not launching satellites, for observation of the paths of the objects can of itself yield results of importance. We can look forward to great activity in this field, reaching further and further out into space without the necessity of manned flight.

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Succulent plants

W. O. JAMES

Succulent plants are found in many very different geographical areas and in many families. Succulence, a development whose purpose is apparently to ensure the plant's survival in areas of uncertain water supply, can occur in any part of the plant and to widely varying extents, although the mechanism by which it is produced is not known. The occurrence and methods of experimental study of succulence are described here.

Succulence consists in the extensive developments of a simple parenchyma in which the cells are thin membranes surrounding drops of water having diameters of the order 10^{-4} cm. The cells tend to be spherical, but may be elongated in some particular direction (figure 14). The degree of succulence is expressed as the ratio of weight to surface. The unit commonly used is the number of grams of contained water per square centimetre of surface. For a given cell weight this ratio has a limiting value. Some cacti and *Conophyta* approach this limit closely (figures 1, 4, and 5). Compared with the diffuse structure more normal among plants this represents a great reduction of surface, and an *Echinocactus* weighing 3 kg was found to have only 1/300th of the surface area of an *Aristolochia sipho* of equal weight.

Succulence may develop in any part of a plant. The more or less spherical cacti have non-succulent roots underground and have reduced their leaves to thorns or eliminated them altogether. They are the most advanced examples of succulence in the stem. Extremes of leaf succulence are achieved by the 'flowering stones', *Conophytum*, *Lithops* (figure 6), and other genera of the Aizoaceae, whose spheroidal bodies consist of two closely appressed leaves and little else. The leaves may even be entirely fused except for a small pore through which the flower emerges (figure 1). Less common is succulence of the flower, which nevertheless is highly developed in those of *Stapelia* species, which have the appearance, more or less the texture, and sometimes the smell, of over-ripe meat. The successful pollination of these flowers depends upon their attractiveness to blowflies (figure 2). All intermediate degrees of succulence leading up to these extreme types exists in different species. An advancing series of leaf succulence is shown in figures 7–10.

Succulence is virtually unknown outside the angiosperms, but within this group it occurs in all members of several large families, notably the Aizoaceae, Crassulaceae, and Cactaceae. It also occurs in many of the Liliaceae and Euphorbiaceae, and more rarely in taxonomically isolated genera such as the composite *Kleinias* (figure 10).

The popular association of succulents with deserts represents only part of the truth. Even the cacti themselves are found in every latitude from British Columbia to Tierra del Fuego. Some *Opuntias* are weeds in North American prairie pastures, where they nestle among the herbage to the discomfort of grazing animals. One species has naturalized itself in the European Alps, and other cacti are found in the high Andes in the snow. The *Sempervivums* (e.g. figure 13) produce their rosettes of succulent leaves in mountain ranges from the Caucasus to the Pyrenees; the *Bryophyllums*, members of the same family, come from the West Indian tropics. Another group of cacti, *Rhipsalis* and the *Epiphyllums* (acclimatized in Victorian parlours), are epiphytes from the Central and South American rain forests. The chenopod succulents (*Salicornia*, etc.) provide the flora of temperate salt marshes. If one seeks for any common factor in such diverse habitats the only unity appears to be in an uncertainty of water supply rather than in a general low level. The 'flowering stones' survive in South African deserts that have been known to receive rainfall only once in three years. The cactuses in their natural sites are very shallowly rooted and apparently derive water only from occasional rains which do not penetrate the desert soils to any depth. It is, in short, not possible to regard succulent plants as a single ecological or physiological type, since they associate their succulence with a diversity of other significant features.

No method is yet known by which any marked degree of succulence can be induced in a normally non-succulent species; but the degree of succulence in a susceptible species can be varied

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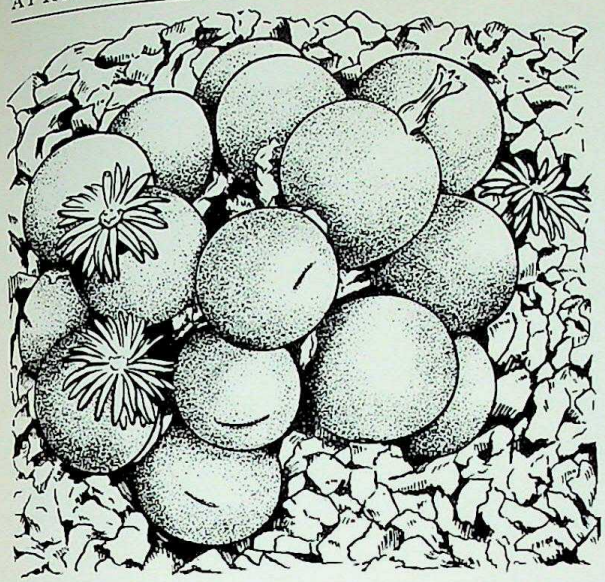

FIGURE 1 – Conophytum calculus. (*about life size*)

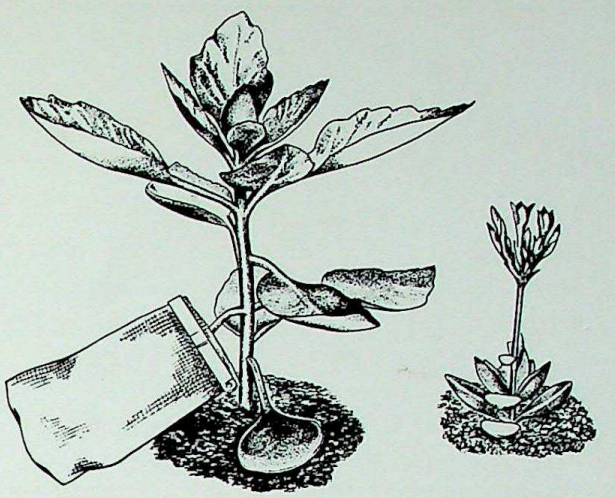

FIGURE 3 – Kalanchoë blossfeldiana. *Left: A long-day plant with one leaf darkened to reduce day-length. Right: A short-day plant.*

experimentally over quite a wide range. The most effective method is by the control of day-length. The crassulacean *Kalanchoë blossfeldiana* grown with days of about 12-hour illumination produces a lax habit with spoon-shaped, only slightly succulent leaves (figure 3), with a succulence ratio of 0·81. On reduction of the day length to 9 hours the leaves become smaller and much thicker, with a succulence ratio of 1·80. Internally, the change consists of an enlargement, particularly in the transverse direction, of the leaf cells, without any considerable increase of cell number (figure 14). It has been shown that if a single leaf of a young *Kalanchoë* plant is subjected to short-day treatment, the leaves that develop vertically above it become succulent: the leaves on the opposite side of the stem retain the normal long-day characters. In this particular species increase of succulence runs closely parallel with acceleration of flowering, but the two effects can be differentiated by mild anaesthetization with chloroform. Under these conditions the acceleration of flowering is abolished but succulence is unaffected.

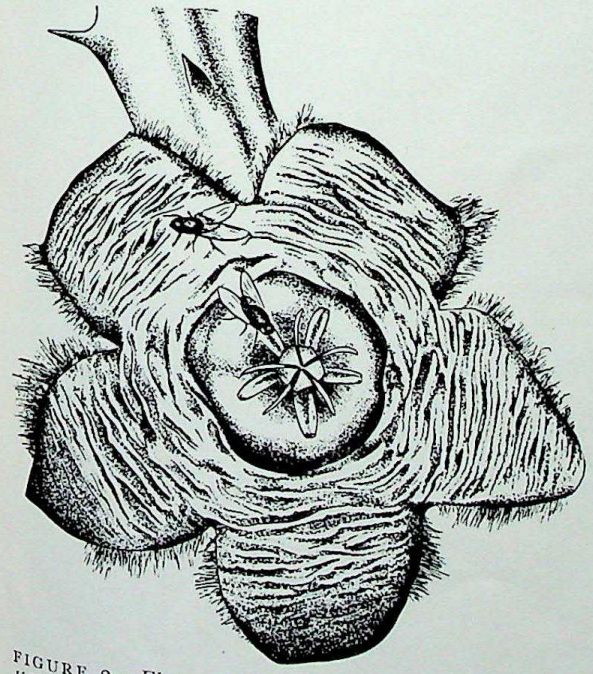

FIGURE 2 – *Flower of* Stapelia comparabilis. (*about life size*)

It would appear that succulence, like the induction of flowering, is determined by a hormone produced in young leaves and conducted upwards to the growing apex. The two effects appear, however, to be due to two different hormones, for other species, such as *Sedum kamschatkicum*, require long days for the production of their flowering hormone but retain the short-day requirement for succulence.

Succulence cannot be induced merely by the prolonged maintenance of a high osmotic pressure within the cells of a growing tissue. Nor does it appear to follow dosage with the growth hormone β-indolylacetic acid, though the effects of the hormone on cell extension have some similarities with the unidirectional type of succulent enlargement. It has been noted that although the chlorophenoxyacetic acids may induce epinasty (a rigid depression of the stalks) in *Kalanchoë* leaves, they do not increase their succulence. The hormone that induces succulence is therefore probably not an auxin.

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FIGURE 4 – Anhalonium lewinii. (*about life size*)

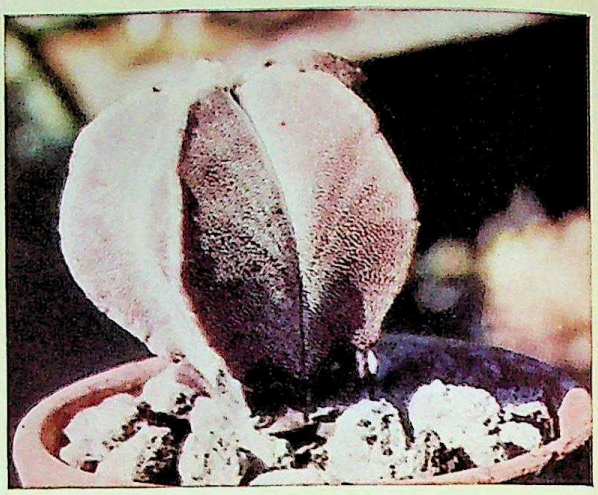

FIGURE 5 – Astrophytum myriostigma. (*about life size*)

FIGURE 6 – Lithops leslei. (*about life size*)

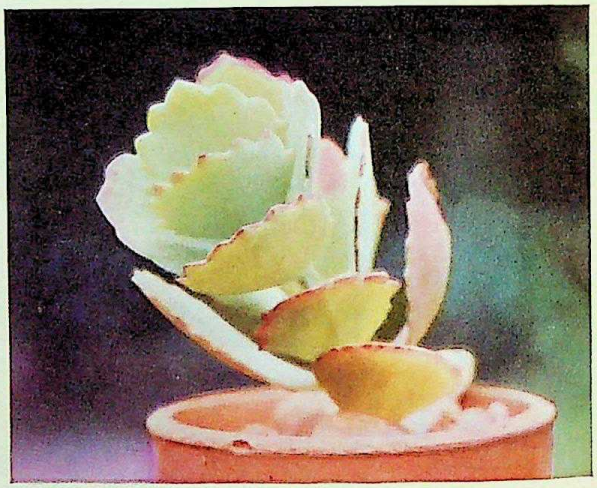

FIGURE 7 – Kalanchoë longifolis. (*about life size*)

FIGURE 8 – Echeveria secunda. ($\times \frac{1}{6}$)

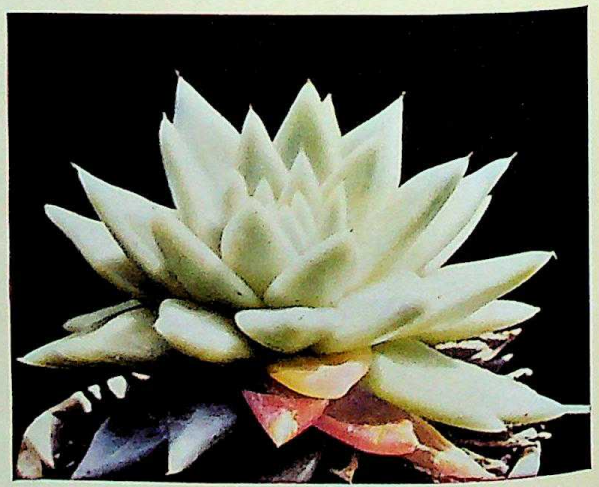

FIGURE 9 – Cotyledon agavoides. (*about life size*)

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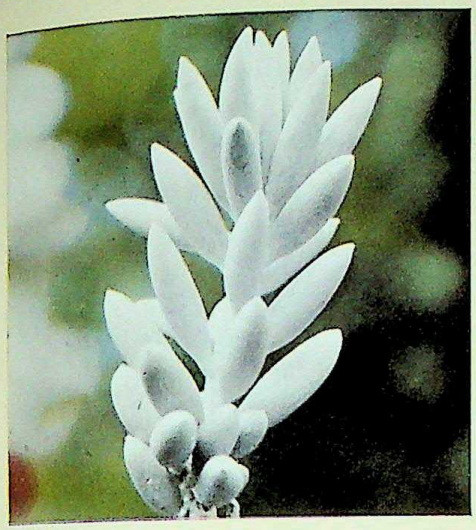

FIGURE 10 – Kleinia tomentosa. *The silvery appearance is due to the fine, closely appressed hairs.* (*about life size*)

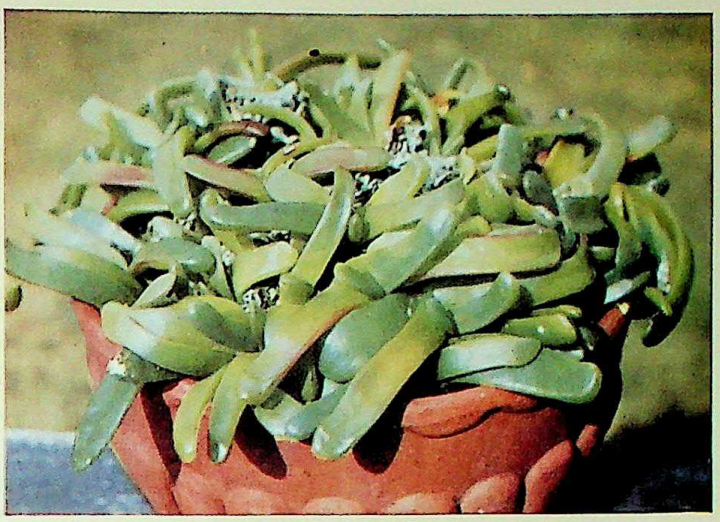

FIGURE 11 – Glottiphyllum linguiforme. ($\times \frac{1}{2}$)

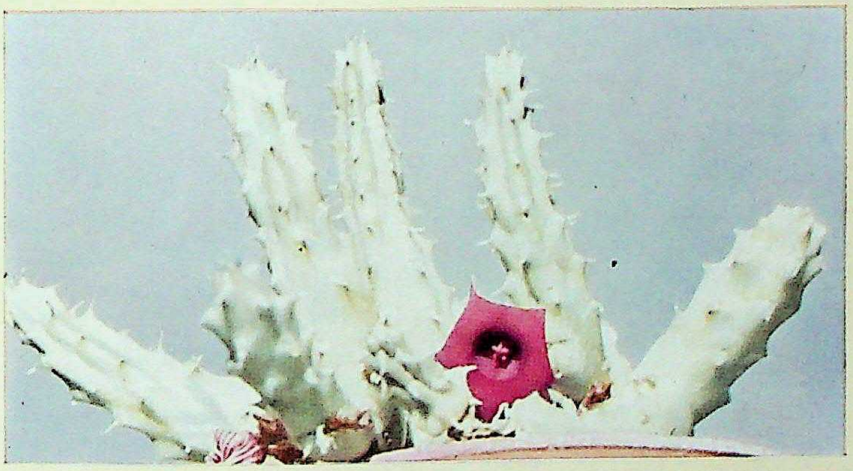

FIGURE 12 – Huernia aspera (*Asclepiadaceae*). (*about life size*)

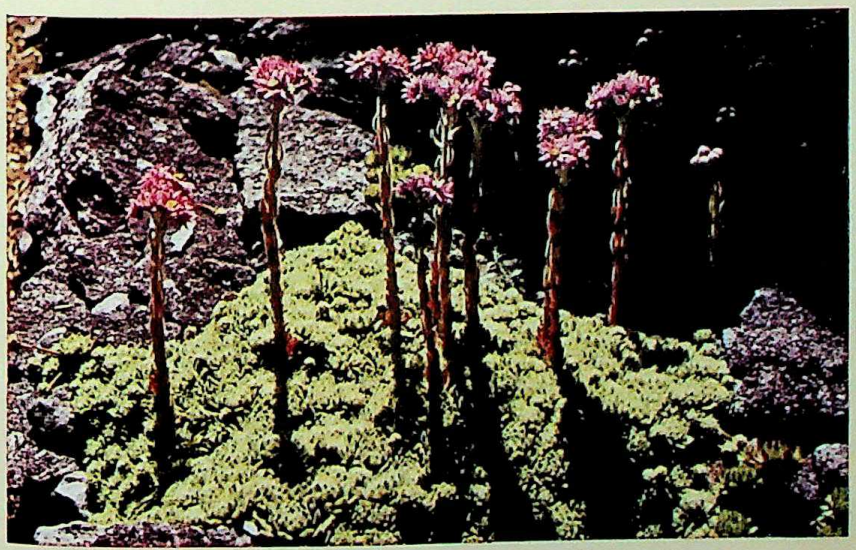

FIGURE 13 – Sempervivum giuseppii. ($\times \frac{1}{2}$)

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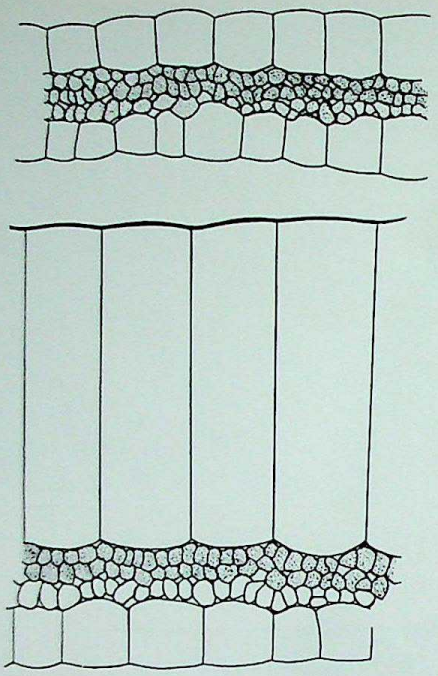

FIGURE 14 – *Transverse sections from* Tradescantia fluminensis *leaves. Top section from a normal leaf; lower section with succulence induced by lack of iron.* (× 50)

The degree of succulence may respond within limits to variations in the supply of certain nutrients, and especially to variations of the K/Ca ratio; but there is not much evidence that succulence may in some way result from a high degree of hydration of the cell colloids. The salt-succulent *Salicornia herbacea* (figure 15) considerably increases its water-content/surface ratio in specific response to the chloride ion, but the ratio is already high before any chloride is added. Significant increases of leaf water content due to chloride have been observed in the non-succulent leaves of potato plants. The causes of succulence have still to be sought among the factors determining cell growth and differentiation, and the mode of operation of the presumed succulence hormone is still quite unknown.

The consequences to a plant's way of life of a succulent habit are somewhat better understood. The most obvious are naturally the effects on its water economy: the essential point here is that succulence does not, in itself, reduce water loss. No plant cell possesses a 'diffusion pressure deficit' (defined as the depression at a given temperature of the diffusion pressure of water in a wet body below that of a flat surface of water under 1 atm. pressure) great enough to offer any serious resistance to the evaporation of its contained water. Normal leaf cells have values of a few atmospheres, which may rise to 20 atm. when the leaves are about to wilt. The diffusion pressure deficit of the air on a temperate day is of the order of 1000 atm. The great majority of succulent cells have maximal diffusion pressure deficits below 10 atm., i.e. below that of non-succulents. This difference has the important consequence that the non-succulent photosynthesizing cells (for example, those shaded in figure 14) will remain turgid, and therefore capable of maximal efficiency, until the succulent water-storage cells have been considerably depleted. It is said that the cells of succulent tissues are able to lose much more of their water without damage to themselves than is usual in other cell types. This sometimes appears to be due to an unusual degree of elasticity in the cell walls, permitting a shrinkage of 20–30 per cent of the cell volume before cell wall and protoplasm part company. This is typical of crassulacean tissues. In the Aizoaceae the walls are less elastic, but collapse into folds still adhering to the protoplasm as the cells shrink. The liliaceous succulents are reported to show a combination of the two devices.

Both succulent and non-succulent tissues alike can exist in a normal atmosphere only if they are protected by a covering having a low permeability

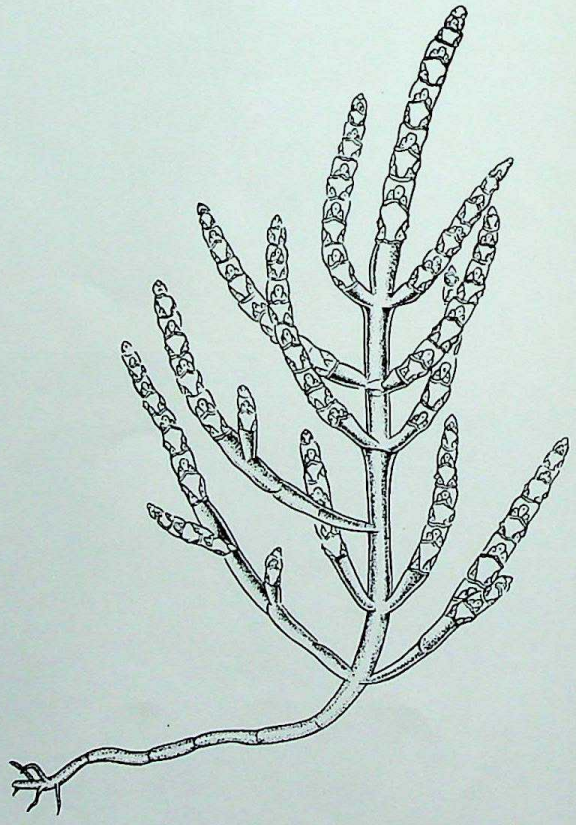

FIGURE 15 – Salicornia herbacea. (× ½)

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for water, and, contrary to what is often supposed, succulent plants differ very much in this respect. The extreme succulents among the Cactaceae have thick impervious cuticles, often with a waxy bloom on the outside. Stomatal openings are comparatively sparse, and it is recorded that in American deserts they tend to shut by day and to open only at night, though this inversion of the normal rhythm does not occur in *Opuntias* grown in Britain. With the degree of surface protection they possess it is not surprising that the cacti lose water only very slowly, even under semi-desert conditions. The rate of transpiration is slow per unit of surface, and the reduction of surface that goes with the advanced succulent habit conserves their water supplies even further. It has been calculated that per unit volume of contained water *Opuntia camanchica* loses water about 1000 times more slowly than, for example, an *Impatiens noli tangere* plant.

But this situation is not general among succulent plants. The *Glottiphyllum* species (figure 11) have thin cuticles and relatively numerous stomata with a normal rhythm of opening. Their transpiration rates are much higher than those of cacti, and they become flaccid rather easily. Even the more advanced types like *Lithops* (figure 6) behave similarly in cultivation, though their behaviour in their natural desert sites is unknown. The succulents of salt marshes, such as the *Salicornia* species, also transpire rapidly and lose their turgidity easily. Their cells acquire relatively high concentrations of inorganic ions from the saline soils which may cause diffusion pressure deficits as high as 40 atm., but this is still negligible in comparison with the value of 1000 atm. for the air.

There are also very marked differences in the capacity of succulent tissues to endure water loss. Cactus stems and aloe leaves can lose 90 per cent of their water and survive. It takes months of drought to produce this condition, whereas *Stapelia* and other asclepiad stems (figure 12) and *Cotyledon* leaves lose the same proportion of their water in three or four weeks and are killed as a result.

An even more remarkable endurance is possessed by the cacti. Owing to their extremely slow transpiration rates there is no cooling effect to offset the temperature rise due to their exceptionally severe insolation. When similar temperature conditions are inadvertently produced in greenhouses the results are commonly fatal to the plants in them. In an air temperature of 31·3° C the sunny side of an *Opuntia* stem was recorded as rising to 63° C (i.e. 15–20° C above the temperature fatal for most plant tissues), without loss of turgor or other signs of damage.

It has been known for at least 150 years that succulent tissues have a marked rhythm of acidity. The acid that fluctuates most within the tissues is malic; and although much of the acid originally supposed to be malic has been proved to be *iso*-citric, this component varies relatively little. Much work and speculation have been devoted to the causes of the malate fluctuation, but it is only comparatively recently that the significant facts have been separated from the mass of data collected. Malate accumulation in crassulacean leaves is favoured by a drop of temperature to about 5° C, i.e. it must result from the adjustment of equilibria. It is accelerated by the absence of light and normally occurs during the night. It can also be accelerated by raising the external pressure of carbon dioxide to about 0·05 atm. Labelling the carbon dioxide with ^{14}C results in the rapid appearance of labelled atoms in the malic and to lesser extents in other acids within the tissues. The formation of the acids is accompanied by consumption of carbohydrates as well as of carbon dioxide, and it now seems clear that it is due to a 'dark fixation' of carbon dioxide, which in light is quenched by the more efficient photosynthesis. The carbon dioxide, once it is fixed in a carbon chain, becomes involved in the complex equilibria of intermediary metabolism: malic acid probably represents the first compound stable under cellular conditions rather than the initial fixation compound. Which of the possible paths to malic acid is most actively in operation has still to be determined.

In *Kalanchoë blossfeldiana* the amount of dark fixation that occurs is much increased by about 21 short day-lengths, and once the enhancement is induced it will continue in long days for at least a year. It has, however, been shown (in unpublished experiments by F. G. Gregory and his associates) that the enhancement of fixation of carbon dioxide does not run parallel with the induction and maintenance of either the succulence or flowering that is also caused by short-day treatment. There appears to be no essential connexion between succulence and acid-formation. Acids accumulate freely, for example, in rhubarb and sorrel leaves, which have no succulent tendencies whatever. Barley roots have been shown to incorporate $^{14}CO_2$ rapidly into malic and other acids; what appears to be characteristic of the crassulacean leaf succulents is the pronounced rhythm of formation and loss. In the succulent stems of *Kleinia articulata*, malate is lost more slowly.

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Research on controlled fusion reactions

By now the main facts contained in the recent joint British and American progress reports on thermonuclear reactions will be generally known. Briefly, it has been demonstrated at three different centres that the exceedingly high temperatures theoretically necessary for thermonuclear fusion in deuterium can be produced in a pinched electrical discharge, stabilized within a toroidal vessel by an axial magnetic field. In Harwell's ZETA, for example, temperatures of the order of five million degrees have been maintained for some thousandths of a second. In SCEPTRE III at Aldermaston temperatures of 4 million degrees have been achieved. At Los Alamos the PERHAPSOTRON has maintained a temperature of 6 million degrees for about one millionth of a second. These brief periods of high temperature can be achieved as often as desired at short intervals. All the experiments show that during these periods neutrons are emitted that are very probably, though not certainly, the result of a thermonuclear reaction with an accompanying liberation of energy. It is believed that this kind of apparatus can be used to obtain substantially higher temperatures at which unequivocally thermonuclear reactions will occur. ZETA is at present being reconstructed for this purpose. The energy released increases rapidly as the temperature rises: at 25 million degrees, for example, it should be at least ten thousand times greater than at 5 million degrees. The scale of the apparatus is important, and the differences in the behaviour of these three machines are due in part at least to differences in the size of the tori. Other things being equal, it appears that the larger the torus the higher the temperature.

Even in this age of great discovery this can fairly be called a milestone in human progress. Man has achieved in the laboratory temperatures higher than that measured for the surface of any star and equal to one-third of the estimated temperature of the centre of the Sun. He has done so by processes of the kind that provide the energy of the stars. This seems, nevertheless, to be only the beginning of a completely new phase in the attainment of high temperatures in the laboratory.

The main interest in these new discoveries is the practical, and exceedingly important, one of utilizing them for the large-scale production of electrical power. At the same time, it is clear that in ZETA and similar apparatuses we have a research tool that will enormously increase our knowledge in many fields of physics and chemistry. If success in power production is attained, and there seems every reason for believing that eventually it will be, man is assured for all time of a supply of power far in excess of any demand that can at present be conceived. But how far ahead this may be is unpredictable, even by those most intimately connected with the various projects. It is certain, however, that years, and perhaps decades, must elapse before any power production on an industrial scale results.

ZETA, it must be emphasized, is an abbreviation for Zero Energy Thermonuclear Assembly: it is so named because the energy released in the supposed fusion reaction is only about one million-millionth of the energy input. For energy output and input to balance, it will apparently be necessary to reach temperatures of about 300 million degrees if deuterium is used; if it were possible to use a mixture of deuterium and tritium, however, a temperature of about 40 million degrees would suffice. The road ahead presents formidable difficulties. New methods may have to be used to heat the gas to these very much higher temperatures and to keep it there for usefully long periods, and new techniques will be required to measure the high temperatures achieved. Beyond this again lie all the tremendous engineering problems of designing and constructing the first thermonuclear power station.

But this is a prospect to fire the dullest imagination, for if success is achieved the result will benefit all mankind. At the same time there are technical difficulties clearly so great as to challange the combined scientific skill of the world. In the experiments that have lately excited so much interest there was over the last year full collaboration between Britain and the United States. But Russian scientists also have for some years past been working in the same field, and published work makes it clear that they too have made progress. There are important research centres also in Sweden, France, Germany and Japan. The International Geophysical Year has demonstrated the possibility of international scientific co-operation on a grand scale: it is very much to be hoped that the harnessing of thermonuclear power too can be achieved on the basis of truly international co-operation.

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Elementary particles and space-time symmetries

A. SALAM

In recent years the number of so-called elementary particles has increased with perplexing rapidity. The purpose of this survey is to systematize, as far as possible, our knowledge in this field, following more or less the historical order of the discovery of these particles and the evolution of concepts associated with them. These concepts are emphasized because they are closely connected with the physicist's notions of the structure of space and time.

The concept of an elementary particle has arisen from man's age-old search for the ultimate, fundamental, and indivisible units of which matter is composed. The nineteenth-century chemist came very near to the end of this quest with the realization, on the basis of the periodic table of the elements, that all matter, of whatever form, was made from 92 different types of atoms. With the work of J. J. Thomson and Rutherford at the beginning of this century came the belief that all these 92 different types of atoms were themselves made from just two elementary particles, the electron and the proton. These are stable, indivisible particles with unique masses and have the following properties:

1. The electron is a very tiny chunk of matter, with a mass of 10^{-27} g; the proton has a mass some 2000 times greater.
2. The electron carries a negative charge of about 10^{-10} e.s.u.; the proton a positive charge of exactly the same magnitude.

The work of Planck and Einstein soon added to this list a third elementary particle, the photon. They recognized that the radiation energy of an electromagnetic field exists in the form of discrete units, which were called photons. On this view, a beam of light consists of a stream of photons, all travelling with the same velocity.

The electron, proton, and photon interact with each other in the following manner:

1. According to Maxwell's classical theory, all charged particles emit or absorb electromagnetic radiation when accelerated: according to Planck and Einstein's view, electromagnetic radiation exists in the form of photons. This means that electrons or protons emit or absorb photons. Figure 1 is a space-time picture of this emission or absorption process. On the right, an electron (solid line) is emitting a photon (dotted line); on the left, an electron absorbs a photon.

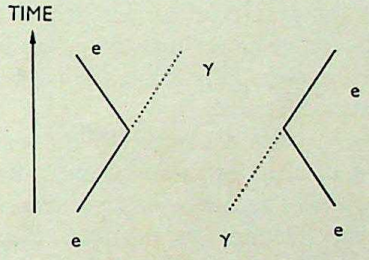

FIGURE 1

2. According to Maxwell's theory, one charged particle attracts or repels another charged particle by first producing an electromagnetic field in the surrounding space, and this field in turn acts on the second charged particle. On this present view this is visualized as an emission of one (or many) photons from one electron and reabsorption by the second (figure 2).

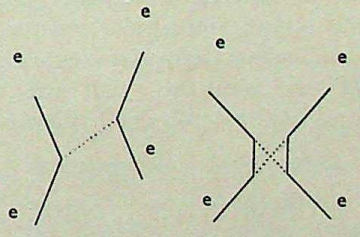

FIGURE 2

3. The emission or absorption of photons and their exchange between two electrons or an electron and proton must proceed in accordance with what are called conservation laws. These require that:
   (i) In any physical process, (*a*) the total charge Q, (*b*) the number of electrons N_e, and (*c*) the number of protons N_p must be the same before and after the interaction.

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The number of photons at the beginning, however, can be different from that at the end.

(ii) In any process, (*a*) the total energy *E*, (*b*) the total momentum P, and (*c*) the total angular momentum J of all the particles must remain the same before and after the interaction.

These so-called conservation laws are experimental. They can, however, readily be linked with ideas about the structure of space-time. One can show that saying that momentum and energy are conserved is equivalent to saying that the results of an experiment are independent of where in space and when in time it is performed. This is the principle of translation symmetry of space-time. It can also be shown that saying that angular momentum is conserved is equivalent to saying that the results of an experiment do not change if the entire experimental set-up is rotated through any angle. This is the principle of rotation symmetry of space.

To give a precise meaning to the concept of conservation of angular momentum, it was found necessary to assign an intrinsic angular momentum (spin) to every elementary particle. Specifically, one has to ascribe a spin of 1 unit to the photon and a spin of $\frac{1}{2}$ unit to the electron and to the proton. To fix one's ideas about intrinsic spin one may roughly conceive of an electron (or a proton) as a spinning top. If the electron is moving, its axis of spin may, to take a special case, be along the direction of motion. In that case, the spin may appear clockwise or counter-clockwise to someone looking along the forward path of the particle. In other words, the electron may move and spin like a right-hand screw (right-polarized electrons) or like a left-hand screw (left-polarized electrons). In a beam of free electrons, half will be right-polarized and half left-polarized (figure 3).

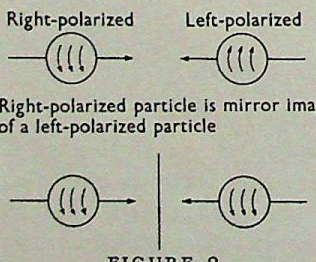

FIGURE 3

Clearly, a right-spinning or a right-polarized electron is a mirror image of a left-spinning or a left-polarized electron. This important point will be referred to later.

To these conclusions about space-time structure P. A. M. Dirac's work carried out during 1928 [1] added a new and deep concept, for he could show on general grounds that all particles in nature must exist in pairs. To every particle there corresponds an anti-particle of precisely the same mass and spin, but of opposite charge. Thus the existence of the negative electron implies the possible existence of a positive anti-electron (the so-called positron); if the proton exists, so must an anti-proton. From the existence of the hydrogen atom we may infer that an atom of anti-hydrogen can exist with precisely the same energy levels.

Further, Dirac showed that when a particle and anti-particle collide both disappear, their energy, momentum, and angular momentum going into photons. Conversely a photon, under suitable circumstances, can produce a pair consisting of a particle and an anti-particle. Figure 4 gives a

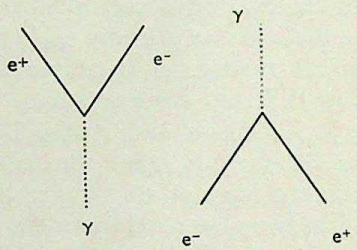

FIGURE 4

space-time picture. On the right, a photon produces a pair consisting of an electron and a positron. On the left, an electron-positron pair disappears with the emission of a photon. To take account of this we must modify two of our conservation laws, namely that N_e = constant and N_p = constant, to read $N_e - N_{\bar{e}}$ = constant and $N_p - N_{\bar{p}}$ = constant. Here $\bar{e}$ denotes positron, $\bar{p}$ anti-proton, etc.

This work of Dirac was among the most momentous in the history of physics. It revealed a deep symmetry in nature. It provided a mechanism by which electron-positron pairs could be created or annihilated. Not long after Dirac's work came the brilliant experimental confirmation of pair-creation by photon showers from C. D. Anderson and P. M. S. Blackett. Figure 5 shows photons passing through a lead plate: being uncharged, they leave no tracks. One can, however, see pairs of oppositely charged particles simultaneously created, curving to the right and left in a magnetic field applied to the chamber in a direction normal to the plane of the paper.

Recapitulating, at this stage we have considered five elementary particles: the photon, the positive

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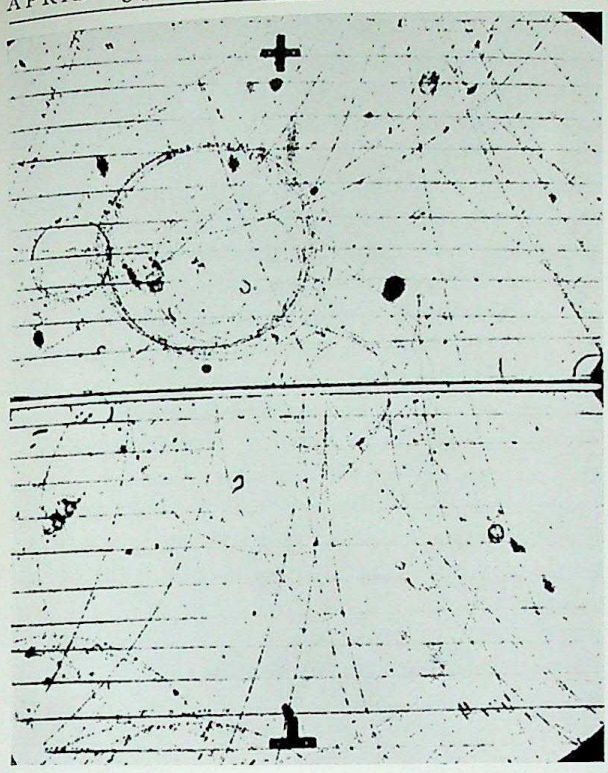

FIGURE 5 – *A shower of protons passing through a lead plate produces electron–positron pairs in the lower part of the cloud-chamber.*

and the negative electron, and the positive and the negative proton. The electron-electron or electron-proton force can be understood completely on the basis of the charge on these particles and an exchange of photons between them.

The next development in the subject came with the realization that the proton-proton force is only partly explained by the above picture. When two protons are close to each other by less than 10^{-13} cm, a much stronger force arises between them, the so-called nuclear force. This force is stronger than the electromagnetic force, produced by exchange of photons, by a factor of 100. At about the same time came the discovery of the neutron—our sixth elementary particle—and realization that all atomic nuclei contain about as many neutrons as protons. The neutron is about as massive as the proton, but differs from it in being chargeless. To a very good approximation the proton-neutron force is equal to the proton-proton force. The neutron and the proton could in fact be considered two states, chargeless and charged, of the same basic particle.

The neutron was destined to enrich the concepts in the subject in a very surprising way. It is slightly heavier than the proton, and in about twelve minutes a free neutron decays into a proton and an electron. In this decay the total energy, momentum, and angular momentum before and after the decay did not seem to balance. Here indeed was a searching test of the theoretical physicist's faith in the concepts he had himself created. If one gave up the demand that these conservation laws should hold, one would have to revise one's ideas about the structure of space-time. To resolve this dilemma W. Pauli suggested that in neutron decay a further neutral particle of zero rest-mass must be emitted which carries away the missing energy, the missing momentum, and the missing angular momentum. This particle was called the neutrino. The discovery of the neutron thus introduced two new elementary particles, the neutron itself and the neutrino.

But is the neutron an elementary particle? The particles we have dealt with so far, the electron, the proton, the photon, and even the neutrino, are all stable, indivisible particles. This is not true of the neutron. To be sure, on the nuclear time scale the neutron decay is a very slow process. The other types of process we have considered, such as photon absorption and emission, take place in about 10^{-19} second. On this time scale neutron decay is certainly very slow. Nevertheless, the fact of the decay remains and forces us into the following compromise. We must divide all fundamental interactions into three classes:

1. Nuclear interactions, which give rise to p–n, p–p, and n–n forces. These interactions are the strongest we know of in nature.
2. Electromagnetic interactions, which give p–e and e–e forces. These are medium strong.
3. Weak interactions, which are responsible for the decay of the neutron.

The relative strengths of these interactions are in the ratio of $1 : 10^{-2} : 10^{-12}$. To the extent that the weak interactions can be neglected, the neutron is elementary, stable, and indivisible. To the extent that the electromagnetic interaction can be neglected the neutron and the proton are identical. Thus all the particles we have considered are elementary, but some are more elementary than others.

Our survey has now brought us to 1935, when H. Yukawa [2] started to ponder over the problem of the specifically nuclear force. We have seen that the electromagnetic interaction of charged particles can be represented as arising from exchange of photons. Yukawa argued that, in complete analogy with this, the proton-neutron force must also be produced by an exchange of some

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new type of particles, which he called mesons. From the characteristics of the nuclear forces Yukawa deduced that:

1. Mesons must possess mass. They ought to be about 300 times as massive as electrons.
2. Unlike photons, mesons may be charged or neutral.
3. Like photons, mesons should be emitted or absorbed singly by the protons or the neutrons.
4. In suitable circumstances one such particle could create a proton–anti-proton or neutron–anti-neutron pair. Conversely, nucleons and anti-nucleons would annihilate each other, the energy and momentum going into mesons (figure 6).

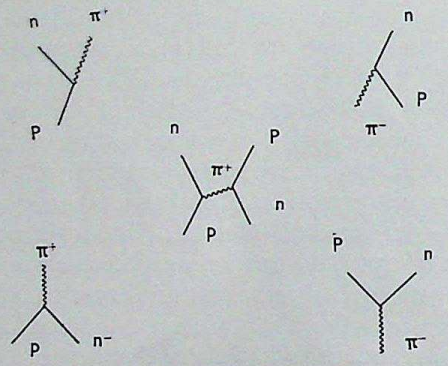

FIGURE 6 – *Solid lines represent protons or neutrons; wavy lines represent mesons. Notice the similarity of meson-nucleon interactions to photon-electron interactions in figures* 1 *and* 2.

The experimental discovery of mesons by C. F. Powell in 1947 forms part of the exciting annals of physics. The Yukawa particles are nowadays called π-mesons (π^+, π^0, π^-, according to charge) (figure 7).

Considering the strong interactions in more detail, at this stage the nuclear force arises between the two nuclear particles (p, n) and the three π-mesons (π^+, π^0, π^-). As remarked before, the nuclear force is much stronger than the electromagnetic. Thus to one part in a hundred, charge on a particle is irrelevant in considering reactions within the nucleus. To a very good approximation, then, the proton and the neutron are identical particles, as also are the three π-mesons. The fact that there are three π-mesons equivalent to each other immediately suggests that we should formally treat these three as components of a vector in a three-dimensional space. To distinguish between this abstract space and ordinary space we call this new space the isotopic space. The equivalence of the three π-mesons is then formally stated as the consequence of rotation symmetry in the isotopic space. As previously noted, rotations are connected with angular momentum, and we can, analogously with ordinary space, assign an intrinsic isotopic 'spin' I_z to π-mesons. On this basis π^+ corresponds to $I_z = 1$; π^0 to $I_z = 0$; π^- to $I_z = -1$. Applying the analogy to the nucleon, which exists in two equivalent states (the proton and the neutron), we may assign $I_z = \frac{1}{2}$ to the proton and $I_z = -\frac{1}{2}$ to the neutron. Just as in ordinary space rotation symmetry implies conservation of angular momentum, so in isotopic space, rotation symmetry implies conservation of isotopic spin.

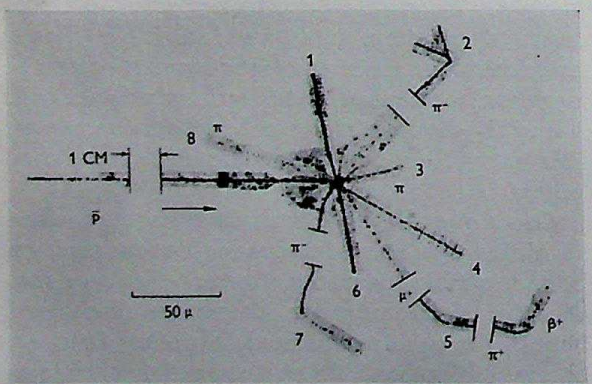

FIGURE 7 – *Example of* $\bar{p} + p$ *collision. A number of* π*-mesons are produced.*

In any nuclear interaction, then, not only energy, momentum, and angular momentum, but isotopic spin should be conserved. But there is one important difference between this new conservation law and the other conservation laws. Clearly the electromagnetic interaction, in so far as it distinguishes charged particles from neutral particles, violates rotation symmetry in this space, and thus the law of conservation of total isotopic spin is only approximate.

Three new particles have now been added to our list. The same question as for the neutron arises again. Are these stable particles? The answer is no, and the decay of all these three particles presents novel features.

Consider π^- first. One might have expected that this particle would decay into an electron and a neutrino. A π^- could convert virtually into an anti-proton neutron pair, and this pair would then disappear, giving an electron and a neutrino. For some very inexplicable reason this does not happen. Instead, nature completely confounds us. The π-meson decays into a new particle, called the μ-meson, and a neutrino. This new particle,

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the μ-meson, is about 200 times as heavy as the electron. The strength of this decay interaction is identical with the interaction responsible for neutron decay (figure 8).

The π-meson mystery does not stop here. In about 10^{-6} second the μ-meson itself spontaneously decays into an electron and two neutrinos. Quantitatively, once again it seems to be the same interaction as is responsible for the π-decay. Without hesitation one may say that the μ-meson is the most mysterious particle in physics. We do not know any good reason why it should exist, nor do we know why it should have such a large mass.

So far we have considered developments before 1947. In the last few years. following a brilliant discovery by C. C. Butler and G. D. Rochester,

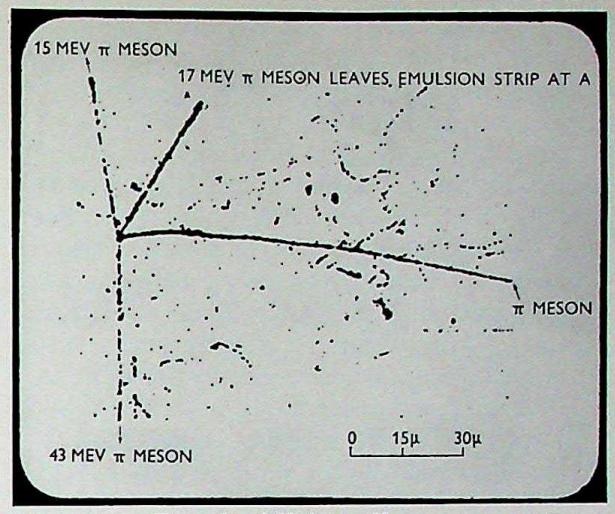

FIGURE 9 – *Example of K-decay,* $K^+ \rightarrow \pi^+ + \pi^+ + \pi^-$.

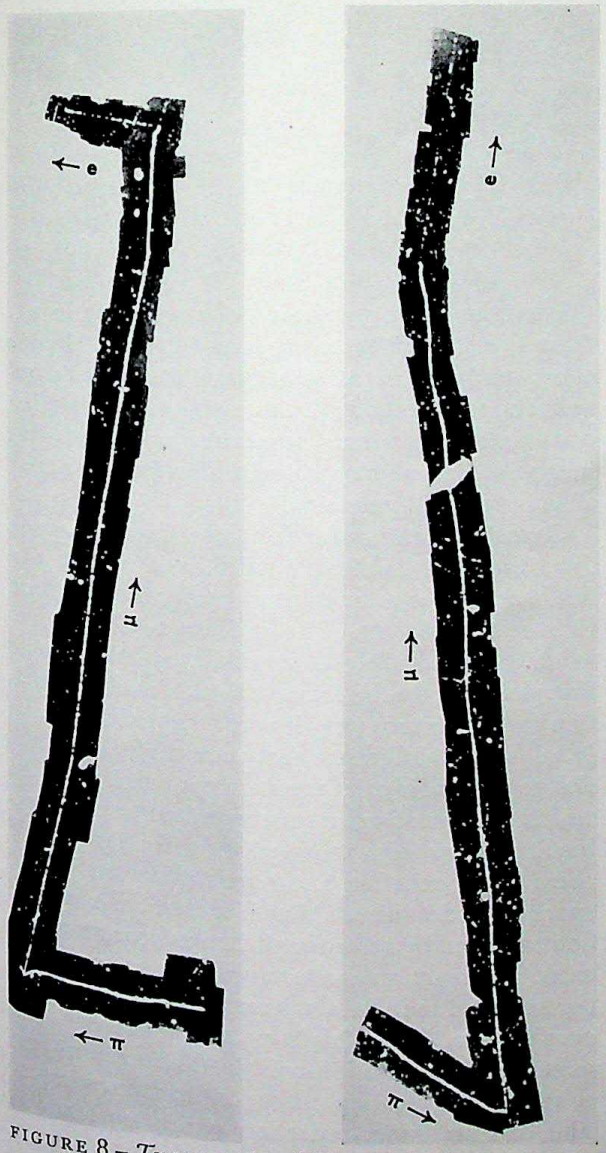

FIGURE 8 – *Two examples of the successive decay* π→μ→e.

eight new particles have been discovered, and we must now consider these. They fall into two distinct categories:

1. There are six particles, each heavier than the proton, and designated Λ^0, Σ^+, Σ^-, Σ^0, Ξ^0, and Ξ^-. These all decay into a proton or a neutron, the decay strengths again being quantitatively the same as are those responsible for n, $\pi^\pm$, and $\mu^\pm$ decays. Since a proton or a neutron is one of the end products in the decay, there still is a conservation law for heavy particles. It now, however, reads $N_p + N_n + N_\Lambda + N_\Sigma + N_\Xi - (N_{\bar{p}} + N_{\bar{n}} + N_{\bar{\Lambda}} + N_{\bar{\Sigma}} + N_{\bar{\Xi}}) = \text{constant}$, where N is the number of particles of the type designated by the subscript.
2. There are also particles, with masses intermediate between the nucleon and the π-meson, which finally decay into electrons and neutrinos. These are the K^+ and K^0 particles (figure 9).

These eight particles presented certain unexpected features and for a number of years were called 'strange' particles. From the copiousness of their production it seemed clear that their mutual interactions were strong in our technical sense. We have seen that isotopic spin is the distinguishing feature of strong interactions. It seemed appropriate to assign to these particles isotopic spin values and to demand that in their interactions isotopic spin conservation should hold. This assignment was made partly on theoretical and partly on phenomenological grounds by M. Gell-Mann and K. Nishijima [3, 4] in 1953. The results were indeed startling. Some of these are:

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1. In any collision process involving π-mesons and nucleons, not only can these 'strange' particles be produced, but there must always be at least two of them. For example, $\pi^- + p \rightarrow \Sigma^- + K^+$.
2. In this same collision it is possible to produce $\Lambda^0 + K^0$, $\Sigma^0 + K^0$, or $\Sigma^- + K^+$, but never $\Sigma^+ + K^-$, even though the total charge, the heavy particle number, etc., all balance on both sides of the reaction. The explanation of this in terms of isotopic-spin conservation law is immediate. We notice from the table that the total I_z value for $\pi^- + p$ is $-1 + \frac{1}{2} = -\frac{1}{2}$. For $\Sigma^- + K^+$, for example, it is also $-1 + \frac{1}{2} = -\frac{1}{2}$. For $\Sigma^+ + K^-$, however, $I_z = +\frac{1}{2}$, and thus this reaction cannot take place. A Pythagorean at this stage may well exclaim 'Number rules the Universe'. As with the former predictions cited, every one of the predictions made by the Gell-Mann-Nishijima theory has been verified (figure 10).

To summarize all our findings:

1. Strong interactions take place between p, n, Λ^0, $\Sigma^{0\pm}$, Ξ^{0-}, $\pi^{0\pm}$, $K^{0\pm}$. These are characterized by the following set of conservation laws (Table I):

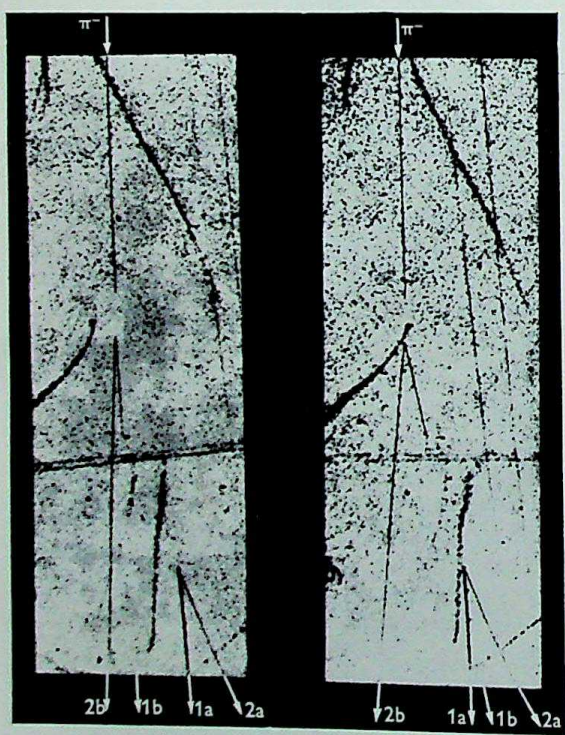

FIGURE 10 – *Associated production:* $\pi^- + p \rightarrow \Lambda^0 + K^0$ *Tracks* 1*a*, 2*a are* p *and* π^- *from* Λ^0 *decay; tracks* 1*b*, 2*b are* π^+, π^- *particles from* K^0 *decay.*

(i) Conservation of charge Q.

(ii) Conservation of heavy particle number $N_p + N_n + \ldots - (N_{\bar{p}} + N_{\bar{n}} \ldots) = \text{constant}$

(iii) Conservation of energy momentum. Space-time translation symmetry.

(iv) Conservation of spin. Space-time rotation symmetry.

(v) Conservation of isotopic spin. Isotopic space rotation symmetry.

(vi) Particle–anti-particle symmetry.

Subject to these rules, any interaction that can take place does take place.

2. Medium strong interactions involve electromagnetic interaction of all charged particles p^+, $\Sigma^\pm$, $\Xi^\pm$, $\pi^\pm$, $e^\pm$, $\mu^\pm$. These interactions are responsible for π^0 and Σ^0 decay in times $\sim 10^{-17}$ seconds. The conservation laws are the same here, save for (v).
3. Weak interactions, which are responsible for the spontaneous decay of all particles except p, e, ν, and the photon. The conservation laws are again (i) to (vi) except for (v), which is peculiar to the strong interaction.

We now come to the most recent act in our drama, bringing us up to January 1957. I have mentioned the translation and rotational symmetries of space-time. I have omitted to mention two further symmetry properties associated with space-time, namely space-reflection symmetry and time-reflection symmetry.

Consider space reflection or mirror reflection first. Since in a mirror a right hand reflects as a left hand, space-reflection symmetry is the same as right-left symmetry. The concept that right and left are indiscernible dates back to Leibniz, who first gave it a precise formulation. From Leibniz's day up to January 1957 it was accepted that there is no inner difference between right and left. Before proceeding, let me state the precise form in which the law of space reflection had been formulated. Just as Dirac showed that for any particle there must exist an anti-particle, so the law of space reflection asserts that if a particle exists the one obtained by reflecting it in a mirror must also exist. If a reaction can take place, the corresponding reaction seen in a mirror is also a physically possible one. Thus if right-polarized neutrinos can exist, so also must left-polarized neutrinos. Just as space-time rotation symmetry leads to conservation of spin, space-reflection symmetry leads to conservation of what is known as 'parity'.

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The space-reflection symmetry or parity conservation principle is philosophically appealing. Even more important than any philosophical argument, the principle is known to hold for all strong and electromagnetic interactions. In the summer of 1956 C. N. Yang and T. D. Lee [5] pointed out that there had until then been no experiment to prove or to disprove it for weak interactions, and they suggested a number of experiments which might clinch the matter. So firm was the belief that the principle must hold for all interactions that W. Pauli wrote to V. Weisskopf on 17th January, 1957: 'I do *not* believe' (the *not* is heavily underscored by the writer) 'that the Lord is a weak left-hander, and I am ready to bet a very high sum that the experiments will give symmetric results.'

The experiments were completed two days after Pauli wrote. They have since been repeated all over the world. They showed unequivocally that in weak interactions there is no right-left symmetry. More precisely, the experiments showed that right-polarized neutrinos exist but left-polarized neutrinos do not. On reflecting a neutrino in a mirror one sees nothing.

It is sobering to think that the experimental results could have been discovered ten years back, for the evidence existed on all the photographic plates recording π^+ and μ^+ decay (figure 8; both photographs are pre-1948). If reflection symmetry holds, relative to the direction of motion of μ-mesons, the same number of electrons should be emitted in the forward as in the backward direction. If anybody had bothered to count the numbers he would have discovered the asymmetry. On January 27th, 1957 Pauli wrote:

'Now after the first shock is over I begin to collect myself. Yes, it was very dramatic. On Monday, the 21st, at 8.00 p.m. I was supposed to give a lecture on the neutrino theory. At 5.00 p.m. I received 3 experimental papers. . . . I am shocked not so much by the fact that the Lord prefers the left-hand but by the fact that he still appears to be left-right symmetric when he expresses himself strongly. In short the actual problem now seems to be the question why are strong interactions right and left symmetric.'

Explaining to a classicist friend the magnitude of the revolution that had occurred in physics, I asked him if any classical writer had ever considered giants with only the left eye. He confessed that one-eyed giants have been described, and he supplied me with a full list of them; but they always sport their solitary eye in the middle of the forehead. In my view, what we have found is that space is a weak left-eyed giant.

One can perhaps give the deeper reason [6] why right-left symmetry should be violated whenever a neutrino is emitted. It can be shown that an exactly zero mass for the neutrino is incompatible with right-left symmetry. We have lost the symmetry principle, but perhaps gained an exactly zero mass for the neutrino. Today the gain seems unimportant compared with the loss, but a few years hence we may think differently.

The time-reflection principle asserts symmetry between past and future; in our formulation it does not make statements about causation but

TABLE I

Comparison of strong, electromagnetic, and weak interactions

	STRONG (10^{-22} sec) p, n, Λ, Σ, Ξ, π, K	ELECTROMAGNETIC (10^{-18} sec) p,$\pi^{\pm}$, $\Sigma^{\pm}$, $K^{\pm}$, Ξ^-, $e^{\pm}$, $\mu^{\pm}$, γ	WEAK (10^{-10} sec) n, π, μ, Λ, Σ, Ξ, K decay
1. Charge conservation	✓	✓	✓
2. Heavy particle conservation	✓	✓	✓
Space-time			
3. Translation symmetry	✓	✓	✓
4. Rotation symmetry	✓	✓	✓
5. Space-reflection symmetry	✓	✓	×
6. Time-reflection symmetry	✓	✓	?
7. Isotopic space rotation symmetry	✓	×	×
8. Particle–anti-particle symmetry	✓	✓	×

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TABLE II

Elementary Particles (1958)

Particle	Rest mass	Spin	I_z	Decay time (sec)	Decay products	Anti-particle
1. γ	0	+1		∞	Stable	γ
2. e^-	1	$\pm\frac{1}{2}$		∞	Stable	e^+
3. μ^-	207	$\pm\frac{1}{2}$		10^{-6}	$e^- + \nu + \bar{\nu}$	μ^+
4. ν^0	0	$+\frac{1}{2}$		∞	Stable	$\bar{\nu}$(spin $-\frac{1}{2}$)
5. p^+	1836	$\pm\frac{1}{2}$	$\frac{1}{2}$	∞	Stable	$\bar{p}$
6. n^0	1839	$\pm\frac{1}{2}$	$-\frac{1}{2}$	10^3	$p + e^- + \bar{\nu}$	$\bar{n}$
7. Λ^0	2180	$\pm\frac{1}{2}$	0	10^{-10}	$p + \pi^-$ or $n + \pi^0$	$\bar{\Lambda}^0$
8. Σ^+	2331	$\pm\frac{1}{2}$	1	10^{-11}	$p + \pi^0$ or $n + \pi^+$	$\bar{\Sigma}^+$
9. Σ^0	2331	$\pm\frac{1}{2}$	0	10^{-18}	$\Lambda^0 + \gamma$	$\bar{\Sigma}^0$
10. Σ^-	2345	$\pm\frac{1}{2}$	-1	10^{-10}	$n + \pi^-$	$\bar{\Sigma}^-$
11. Ξ^0	2590	$\pm\frac{1}{2}$	$\frac{1}{2}$	10^{-10}	$\Lambda^0 + \pi^0$	$\bar{\Xi}^0$
12. Ξ^-	2590	$\pm\frac{1}{2}$	$-\frac{1}{2}$	10^{-10}	$\Lambda^0 + \pi^-$	$\bar{\Xi}^-$
13. π^+	273	0	1	10^{-8}	$\mu + \nu$	$\pi^-(I_z = -1)$
14. π^0	264	0	0	10^{-16}	2γ	π^0
15. K^+	966	0	$\frac{1}{2}$	10^{-8}	$\mu + \nu$, 2π, 3π	$K^-(I_z = -\frac{1}{2})$
16. K^0	966	0	$-\frac{1}{2}$	10^{-10}	$\pi^+ + \pi^-$ or $\pi^0 + \pi^0$	$\bar{K}^0(I_z = +\frac{1}{2})$

merely such statements as that: The number of K's and Σ's produced in $\pi^- + p$ collision is the same as the number of $\pi^- + p$ produced in K, Σ collision. It is known that the principle holds in strong interactions. We strongly suspect that it also holds for electromagnetic interactions. There is no experiment yet to test it for weak decays. One may remark that if the principle holds in weak decays, the left-eyed giant, on looking into a mirror, will see not a right-eyed giant but a right-eyed anti-giant.

DISCUSSION

The entire development presented here is based on the assumption that the structure of space and time is that given by the special theory of relativity. The reason for ignoring the general theory of relativity is that the gravitational force is an even weaker one than any that have been considered. It has a strength of 10^{-34} in the units used above, and to an excellent approximation its effects can be neglected.

Turning to some of the problems in the physics of elementary particles, we ask why there are just these particles. Are there still more particles to be discovered? In the case of particles possessing strong interactions we believe we understand the deeper reason for their existence in terms of the isotopic spin space. Why this space exists we do not know; but, granted its existence, it would seem that we have already discovered all particles possessing strong interactions, except possibly for one particle. This is a rash statement. Such statements have been made again and again in the history of physics and have always proved false, and I must qualify it by saying that the isotopic group may admit other particles, but they will all probably have lifetimes shorter than 10^{-19} second.

We still do not know the deeper symmetry principle associated with particles falling in the electromagnetic and weak interaction categories. There may well, for example, be more companion particles of the μ-meson. In fact, all we know about the particles that fall in these two categories is that their interactions violate some of the strong interaction symmetries. These interactions seem in a sense to have a negative role. There must in nature be a hierarchy of symmetry principles, some of which are dearer to nature's heart than others.

All our remarks on whether further particles are likely to be discovered or not depend, of course, on whether there exist further categories of interactions besides the three mentioned. If there do, naturally there will be whole classes of new particles. This question is connected with the existence of conservation laws. So far, we believe that four conservation laws hold universally. Are there

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further interactions weaker still, for which these also break down? Cosmologists in their theories of continuous creation have already suggested that energy and momentum are indeed not conserved when the still weaker gravitational interaction is considered. From the present point of view this hypothesis becomes entirely plausible.

On looking through the table of elementary particles there is only one thought in my mind: how deeply privileged our generation is to have been presented with this fascinating challenge. People speak of the multiplicity of elementary particles—they even give them outlandish names like 'strange' particles; they shake their heads in disapproval of 'weak' laws. I believe these are but stepping stones to an inner harmony, a deep pervading symmetry. The μ-meson may seem out of place today. When we discover its real nature we shall marvel how neatly it fits into the Great Scheme, how integral a part it is of something deeper, more profound, more transcending. Faith in the inner harmony of nature has paid dividends in the past. I am confident it will continue to do so in the future.

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Book reviews

QUATERNIONS

Five Mathematical Structural Models, *by Otto F. Fischer. Pp. vi + 412. Axion Institute, Stockholm.* 1957. $10 *net.*

The purpose of this book is to illustrate the use of quaternions in the various branches of mathematical physics. The author shows how well they are adapted for the discussion of Maxwell's equations, but the reviewer must confess that when it comes to an application to the theory of elasticity he is inclined to think that the analysis is clearer when expressed in the customary tensor notation. There are other interesting applications to wave mechanics, to quantum theory, and to Hamilton's optics. The book is a little difficult to read, partly because it is reproduced by the rotaprint process and partly because there are so many diacritical signs attached to the symbols for the quaternions. There is an impressive list of references, but it does not include the pioneer work on quaternions by McAuley.

G. TEMPLE

DYNAMIC INSTABILITY

Dynamic Instability, *by Y. Rocard. Translated from the French by M. L. Meyer.* Pp. *xi* + 227. *Crosby Lockwood & Son Ltd.*, London. 1957. 45s. *net.*

University lecturers and experts in industry will welcome the English translation of this excellent work. Anyone who requires a general introduction to the field of dynamic instability will find this developed in a very logical manner and amply illustrated with a number of highly topical problems.

The criterion for instability is derived for examples with two degrees of freedom. When two separate oscillating systems, each with its own natural frequency, are coupled together by a spring, two new frequencies of oscillation arise. The difference between these new frequencies is always greater than the difference between the original frequencies. Such a coupled system is conservative and stable, and always obeys the Lagrange equations. Introduction of external forces independent of elastic potential but in phase with either the displacement or the acceleration vectors, i.e. a feedback of energy, reduces the difference between frequencies of coupled systems. Instability is imminent when these frequencies become equal. The whole book is based on this theorem, which is derived most lucidly for simple systems and applied in detail to the stability calculations for motor cars, including wheel shimmy and, for aircraft, wing flutter and longitudinal motion. The problem of self-excited oscillations due to wind forces of suspension bridges is dealt with in great detail (about half the book is devoted to this), with particular reference to the Tacoma Narrows Bridge. The book is well produced, and the index would appear to be adequate.

P. GROOTENHUIS

QUANTUM MECHANICS

Quantum Mechanics, *by H. A. Kramers. Pp. xvi* + 496. *North-Holland Publishing Co., Amsterdam.* 1957. *Fl.* 45 *net.*

We are told that the frontiers of science advance at an ever-increasing rate. It is therefore reassuring to find an account of the fundamentals of quantum mechanics written in 1937, which is as readable now as it must have been then—and almost as up to date. The author has a most intriguing way of easing rather than plunging the reader into the intricacies of the subject, and nothing of this is lost in a most English translation into English. The book is sprinkled with original proofs and delightful asides. An example of the former is the proof of the existence of the energy eigenvalues of the Schrödinger equation, of the latter the remark on the thorny problem of the connection of spin and statistics that 'the argument is not rigorous but contains some grains of truth'.

Where the book tends to date is in its treatment of quantum electrodynamics. This subject has changed so radically in the post-war years that any pre-war treatment of it looks decidedly clumsy. Quantum electrodynamics has long lost its 'preliminary character', and a footnote referring to Thirring's book, published in 1955, is ingenuous rather than helpful. But this is a splendid book, and English physicists owe

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much to Dr ter Haar for undertaking the selfless task of translation.

L. R. B. ELTON

PIEZOELECTRICITY

Piezoelectricity. *Pp. x + 369. Her Majesty's Stationery Office, London.* 1957. *75s. net.*

This is the first of the Selected Engineering Reports of the Post Office Research Station and comprises eleven papers on certain aspects of piezoelectricity important in the design of crystal oscillators, resonators and similar electro-acoustic devices. R. Bechmann, a prominent worker in this field, was responsible for most of the work described.

The first four papers treat theoretically the fundamental relationships between the elastic and the electrical properties of crystals and the determination of the relevant constants. Other papers describe practical measurements of the constants of several water-soluble piezoelectric crystals, and there is an interesting account of the growth and cutting of large artificial crystals.

This is a book for the specialist. It brings together much information at present scattered throughout the literature and provides a handy reference source for the research worker engaged in the crystal vibrator field. The earlier papers can be read with profit by those interested in the theoretical aspects of piezoelectricity; the treatment is lucid and refreshing and is as simple as this difficult subject permits.

It is a pity that the summary of the published data on piezoelectric materials does not include some of the recent piezoelectric materials now in demand.

H. J. H. STARKS

ION-EXCHANGE RESINS

Ion-Exchange Resins, *by J. A. Kitchener. Pp. vii + 109. Methuen & Co. Ltd., London; John Wiley & Sons Inc., New York.* 1957. *9s. 6d. net.*

This is the latest of the Methuen Chemical Monographs, and it maintains in every way the high standards of this series.

The book is intended to be a concise introduction to the theory and general fundamentals of ion-exchange resins, and in this it succeeds admirably. Unlike the majority of books on ion exchange, it is not overburdened with literature references, and there are merely a few selected 'key' references at the end of each chapter. These are thoroughly up to date: over three-quarters of them are subsequent to 1952, while one is as recent as 1957.

The book begins with a brief account of the discovery of the phenomenon of ion exchange and the development of exchangers from the inorganic silicate materials to the resins. The fundamental physical chemistry of exchange equilibria and kinetics follows, and a few selected applications of the resins are then given. The final chapter describes some recent developments, including highly selective and redox resins and ion-exchange resin membranes. The style is flowing and is distinguished by a complete absence of padding. There is in consequence a vast amount of information concentrated into the 100-odd pages.

The subject matter is pitched at just the right level to be understood by the newcomer and yet it is thoroughly readable by those already working in the field. To those in both these categories the book can be highly recommended.

R. E. KRESSMAN

TOXIC PHOSPHORUS AND FLUORINE COMPOUNDS

Phosphorus and Fluorine. The Chemistry and Toxic Action of their Organic Compounds, *by B. C. Saunders. Pp. xv + 231. Cambridge University Press, London.* 1957. *32s. 6d. net.*

This monograph stems from wartime research on toxic compounds containing phosphorus and fluorine, in which the author played a leading part. His subject has now, however, a much wider interest, for many compounds of the types discussed are of great value in pest control, and a few have applications in the field of clinical medicine. Not least, work on the mode of toxic action of these substances has resulted in important advances in our knowledge of enzyme systems.

The chemical sections of the book describe the preparation and reactions of various groups of toxic compounds in which the author has been interested. Chief among these are the fluorophosphanates, or phosphofluoridates as they are now more usually called, and fluoracetates, together with numerous substances related to them. No attempt has been made to cover the field completely, however, and it would have been impossible to do so in a book of this size. Instead the author has sought to discuss in some detail the more biological aspects of his subject. This he is well qualified to do, and it is this dual approach that gives the book its special value. The non-specialist reader is given a general account of the mammalian nervous system and then of current ideas on enzyme activity and inhibition. The result of this new approach is a book that can be given an unqualified recommendation. It will interest not only chemists, but also many biologists who are concerned with the problem of enzyme inhibition.

H. J. EMELEUS

CHEMISTRY OF THE ACTINIDE ELEMENTS

The Chemistry of the Actinide Elements, *by J. J. Katz and G. T. Seaborg. Pp. xv + 508. Methuen & Co. Ltd., London; John Wiley & Sons Inc., New York.* 1957. *63s. net.*

The actinide elements include all the nuclear fuels and fuel precursors of atomic energy. It is most valuable, therefore, to have this comprehensive and authoritative account of their chemistry from distinguished members of two of the principal centres where they are studied. It will to a large extent replace the earlier volume affectionately known as '14A' in the American National Nuclear Energy Series, which has by now become somewhat out of date. The new book is up to date enough to include a passage on nobelium, element 102, which was discovered last spring.

There is an astonishing amount of information packed into the book, often in tabular form. The authors have been most successful, too, in summarizing the varying views on issues still in doubt.

The plan of the book, after a brief introduction, is to consider the elements in order, chapter by chapter, starting with actinium. For each element there is a brief historical account; a survey of its isotopes; a section on its natural occurrence (where appropriate); a fairly full description of methods of extraction or preparation and purification, including industrial processing; and finally an account of its chemistry. The last, long chapter of the book summarizes and correlates properties throughout the actinide series.

In a sense, the actinides are treated as if they were ordinary, non-radioactive chemical elements, for there is very little on radiochemical techniques, other than a couple of pages and four photographs in the introduction, dealing with problems of handling. The omission is obviously deliberate, to keep the book within reasonable limits.

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The book is well printed, gives full references, and has a comprehensive index.

H. A. C. MCKAY

ORGANIC CHEMISTRY

Tetrahedron. The International Journal of Organic Chemistry, *Vol.* 1, *Nos.* 1–2. *Pp.* 176. *Pergamon Press Ltd., London.* 1957. *Subscription per volume,* £6; *for subscribers certifying that the journal is for their own personal use,* £3 10*s.* net.

Sir Robert Robinson, the founder of this new journal, writes in the foreword: 'The special character claimed for *Tetrahedron* is its fully international scope, since the new journal is envisaged as a forum for the presentation of original memoirs on organic chemistry contributed from all parts of the world.'

The composition of the Board of Honorary Regional Editors shows the international character: Professor M. S. Kharasch (Chicago), Professor R. H. Martin (Brussels), Professor A. N. Nesmeyanov (Moscow), Professor F. Weygand (West Berlin). The Executive Editor is Professor H. Stephen (London). The Honorary Editorial Advisory Board comprises 41 organic chemists from Europe, the American continent, and the Far East.

An international journal on organic chemistry with articles in English, German, and French will fill a gap. It provides the organic chemist with a forum which other sections of chemistry have already at their disposal in international periodicals.

'Tetrahedron' accepts not only original papers of the usual type, but also 'longer papers in which the results of extended investigations are described in a unified manner'. This first issue contains summarizing articles by G. Fodor, A. P. Terentiev and V. M. Potapov, P. J. C. Fierens and S. Berkovitch, and A. N. Nesmeyanov and others. It also includes 15 contributions, mainly on synthetic investigations of substances like alicyclic compounds, steroids, heterocyclic compounds, and alkaloids.

J. P. WIBAUT

PRACTICAL FOSSIL CLASSIFICATION

Détermination pratique des fossiles, *by A. Chavan and A. Cailleux. Pp.* 387. *Masson et Cie, Paris.* 1957. *Paper covers, Fcs* 5000; *bound, Fcs* 5800 *net.*

About 100 years ago Bentham, in his 'Handbook of the British Flora' devised artificial keys for the naming of plants. They comprise a series of two (rarely more) alternatives as nearly as possible contradictory. Having determined which of the first alternatives fits the specimen, the inquirer is referred to another pair of alternatives, and so the sequence proceeds by dichotomy until eventually sufficient alternatives have been worked through to lead to a single generic name.

The authors have attempted a similar scheme for fossils, both animal and plant, and their plea is that palaeontology nowadays demands so much specialized knowledge that the amateur requires an easier method of identifying his fossils than labouring through large monographs. Like Bentham's analytical keys, those devised for fossils call for no more than a rudimentary understanding of morphology; the approach to the subject is mechanical, but possibly useful to those who look on fossils merely as objects showing infinite variety of form. There is no opportunity to evaluate relationships, no attempt to make a fossil 'live', no appreciation of its position in an evolutionary series, and no assessment of its value as an index to geological age, although the geological range is usually given. In fact, the book has little on those subjects for which many amateurs show the keenest curiosity and interest.

W. F. WHITTARD

EXTRACTS FROM DARWIN'S WORKS

The Darwin Reader, *edited by Marston Bates and Philip S. Humphrey. Pp.* ix + 481. *Macmillan & Co. Ltd., London.* 1957. 30*s. net.*

It is undoubtedly extremely desirable that not only students of science but also the majority of well-educated persons should be acquainted with some of the classical scientific writings which have formed our present-day mode of envisaging the world we live in. However, this laudable aim is not very easy to achieve. How much time can most people spend in reading quite lengthy works, many of whose details may have been superseded, even though their main conclusions have grown in importance? Darwin is so much the focus of perhaps the most revolutionary change in 'common sense' that science has ever brought about that it is he, above all others, whom one would wish to see widely read. One can therefore be most grateful to Professors Bates and Humphrey, of the University of Michigan, for their admirable selection from his voluminous writings. Their extracts are introduced by some 100 pages from his Autobiography and 'The Voyage of the *Beagle*.' The 'Origin of Species', reduced to about a third of its full length, is prefaced by the Darwin-Wallace essays published by the Linnean Society; and the book concludes with selections from 'The Descent of Man', 'The Expression of the Emotions', and short extracts from the papers on climbing and insectivorous plants, and on the activities of earthworms. There is a short selected bibliography and an adequate index.

C. H. WADDINGTON

THE NATURE OF A SPECIES

The Species Problem, *edited by Ernst Mayr. Pp.* ix + 395. *American Association for the Advancement of Science, Washington; Bailey Bros and Swinfen Ltd., London.* 1957. 80*s. net.*

Nearly a century ago Darwin and Wallace showed that species arise by evolution, but uncertainty still reigns as to how a species should be defined. In the past, taxonomists have relied mostly on the morphological criterion of a group of organisms that resemble one another more closely than they resemble any other organisms. This criterion is often the only one that can be practically applied, particularly on preserved material and fossil specimens. It suffers from the criticism that the amount of morphological difference that entitles a population to the status of a species is an arbitrary decision of the taxonomist.

The biological concept of the species is free from this criticism, for it considers the species as a group of organisms prevented from reproducing with other organisms by fertility barriers which may be ecological, genetical, or geographical, and can be tested and settled by observation and experiments on living populations in nature; but this criterion is difficult to apply to organisms that reproduce asexually, and to fossils. Yet another concept of the species is as a 'gene-pool' of such a kind that an hereditary character of any individual can potentially be transmitted to any other individual of a later generation, in any part of its geographical range.

These remarks will suffice to show that there is such a thing as the 'species problem', and that it is very important. Dr Ernst Mayr is universally recognized as a leading authority on these problems, and this work should be accessible in all scientific institutions where they are taught and investigated.

GAVIN DE BEER

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BIOCHEMICAL ANALYSIS

Methods of Biochemical Analysis, *Vol. V, edited by David Glick. Pp. ix + 502. Interscience Publishers Inc., New York; Interscience Publishers Ltd., London.* 1957. $9.50 *net.*

Biochemical techniques of analysis are of ever-increasing complexity and are being developed at a very rapid rate. It is therefore useful to have this annual series, now in its fifth year, which presents monographs on procedures for the determination of biologically important substances. The present volume of eight contributions deals with the assay of cholinesterase (K. B. Augustinsson); biological (i.e. antibiotic, hormonal, enzymic, pharmacological and immunological) standards in biochemical analysis (J. H. Humphrey, D. A. Long and W. L. M. Perry); α-keto acid determination (W. J. P. Neish); microdetermination of cobalt (B. E. Salzman and R. G. Keenan); activation analysis ('activation analysis' rests on the fact that many elements may be determined in minute quantities by measuring the radioactivity induced when the sample is exposed to bombardment by nuclear particles) in biochemistry (B. A. Loveridge and A. A. Smales); contamination in trace element analysis and its control (R. E. Thiers); chemical determinations of oestrogens in human urine (W. S. Bauld and R. M. Greenway); infra-red analysis of vitamins, hormones, and coenzymes (H. Rosenkrantz). The monographs are detailed and competent. They give adequate working instructions together with critical comments and extensive references to the literature. The volume is a very valuable addition to the reference books on modern biochemical techniques.

H. A. KREBS

TEXTBOOK OF ZOOLOGY

Précis de biologie animale, *by M. Aron and P.-P. Grassé. Pp. viii + 1413. Masson et Cie, Paris.* 1957. *Paper covers, Fcs* 5300 *net; bound, Fcs* 5900 *net.*

This book is designed to meet the requirements in biology of French students working for the certificate in chemistry, physics, and biology, a standard which they must successfully attain at their university before they can start their studies in medicine, pharmacy, or dentistry. This certificate, which now involves a new syllabus, corresponds, therefore, to the British First M.B. examination, and it is interesting to compare this book with its British equivalents. Here it need only be said that this book would form an admirable handbook for an honours course in zoology. Of particular interest are the space and treatment given to such subjects as viruses, genetics, cellular permeability, the chemistry of respiration, enzymes, hormones, dietetics, nervous excitation, and evolution. The portion of the book devoted to the structure and development of the various groups occupies 624 pages, less than half the total. Seldom has so much reliable and up to date information been contained in one handy and attractive volume. Its price, however, equivalent to about £5, makes it difficult to see how students can hope to possess their own copies.

GAVIN DE BEER

HISTORY OF THE PHYSIOLOGY OF PAIN

Anatomies of Pain, *by K. D. Keele. Pp. x + 206. Blackwell Scientific Publications, Oxford.* 1957. 27*s. net.*

Dr Keele endeavours to persuade us in the first half of his book that those who are now concerned with the study of the physiology of pain would benefit substantially by considering a review of what has been said on this subject from the earliest times.

My own belief is that, fascinating as the study of the history of physiology before the nineteenth century may be, it contains nothing of the slightest use to modern physiologists, and Dr Keele's book only serves to strengthen me in this conviction. What the ancients might have to say about their subjective experiences of physical pain would still be of interest, but if any such records exist, Dr Keele does not seem to have found them. This is not said to impugn his industry, which has been beyond praise: but in dealing with ancient authors, industry is not enough.

The second half of Dr Keele's book, which reviews studies of pain since about 1800, is undoubtedly of relevance to present work, and had the first half been omitted the author could have expanded and improved to great advantage what remained.

J. S. WILKIE

AGRICULTURAL ECONOMY

Types of Rural Economy. Studies in World Agriculture, *by R. Dumont. Pp. xii + 556. Methuen & Co. Ltd., London.* 1957. 45*s. net.*

This is a very good English translation of a long and detailed work by a professor of agricultural economy in the University of Paris. It consists of a series of case studies of individual farms and local agricultural areas in different parts of the world, from the Belgian Congo in the south to England in the north, and across as far as northern Vietnam in the east. These cases, not necessarily typical or average, but which arose from the author's personal experience, are used to illustrate what the author considers to be the major agricultural problems of large areas of the world. They are used, in particular, to contrast the problems of backward and poor farms as against progressive holdings and constructive national agricultural policies. Out of this come, for example, appreciative comments on recent English farm policy and strong criticism of French farming practice.

The value and strength of the book lie in the pen portraits of individual farms across the world, drawn in terms of the ways in which the farmers actually live. This will be particularly valuable to students of agriculture in countries where there is little local agricultural literature, for example those studying in the new University Colleges in Africa. A possible weakness of the book lies in the matrix of general argument into which the chunks of detail are embedded. This argument is somewhat long-winded and interspersed with factual information which does not always give clarity. The author certainly emphasizes important basic problems and their recurrence among countries. These problems include those of smallness of farm businesses, the extensive use of good land, the over-intensive use of poor land, the benefits of farm mechanization, the effects of population pressure on rural land use, and the poverty and weakness of heavily agrarian countries or regions. Yet how fascinating it is to have these problems discussed by a Frenchman rather than by a Briton or American.

G. P. WIBBERLEY

PEST CONTROL

Advances in Pest Control Research, *Vol. I, edited by R. L. Metcalf. Pp. vii + 514. Interscience Publishers Inc., New York; Interscience Publishers Ltd., London.* 1957. $11 *net.*

Few fields of human endeavour embrace such a range and depth of the scientific disciplines as that of controlling our fungal, weed, insect, and other biological rivals. The first volume of the new series 'Advances in Pest Control Research', edited by Dr Metcalf, is therefore very welcome and fulfils a real

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need. In relation to insect control this need has been partly met by the new 'Annual Review of Entomology', and it is hoped, therefore, that the slight danger of wasteful overlapping in this area will not be overlooked by the respective editors of future volumes of both series.

The first volume of 'Advances' deals with the following subjects: Health hazards associated with chemical pesticides, herbicidal action, fungicidal action, organophosphorus insecticidal action, control of soil fungi, use of systemic insecticides, development and application of insect repellants, chemical and biological assay of pesticide residues, and the use of radioisotopes in pesticide research. Without exception the contributors are recognized authorities within their fields. Unfortunately their clarity and style of writing are sometimes unsatisfactory. Thus the definition of a systemic insecticide (pp. 305–6) is more of a confusion than a definition; the frivolous style of one contributor may irritate the reader who finds such remarks as 'I shall have fun being unorthodox again' (p. 199) a little misplaced in a scientific review; while the use of coined terms, strange expressions such as 'enzymatic metabolization' (p. 308), or unexplained and ambiguous abbreviations such as 'IAA' (p. 59) may also displease the reader. Despite these blemishes the book contains a wealth of useful information and, in parts, provides stimulating reading. This first volume and the succeeding volumes should find space on the shelves of all those concerned with pesticide research and development.

F. P. W. WINTERINGHAM

TREES IN INDIA

Flowering Trees in India, *by M. S. Randhawa. Pp.* 210. *Indian Council of Agricultural Research, New Delhi.* 1957. 15 *rupees net.*

The mere title of this book, written by Dr Randhawa, Vice-President of the Indian Council of Agricultural Research, gives no hint of the wealth of information that is contained between its covers. It consists of a series of well-written essays on such diverse topics as trees as artistic subjects, folklore of trees, plant migration, landscape planning, *Vana Mahotsava* or the Indian National Festival of Trees, trees for towns, main roads, the garden, and much more besides. The book is lavishly illustrated by coloured plates, some of which are excellent; others can be appreciated only by those who possess an artistic soul, a gift which the reviewer unfortunately lacks.

The man who stands revealed in this book is that unusual combination of idealist and hard-headed pragmatist, one who can see the wood as well as the trees. India is lucky to have him.

N. L. BOR

HISTORY OF SCIENCE

Histoire des sciences, *by Fernand Perrin. Pp.* 605. *Editions Beaudart, Paris.* 1957. *Fcs.* 4315 *net.*

It is improbable that this history can replace the variety of works in English that describe in outline either the whole evolution of science or that occurring in a limited period. It is in fact little more than a collection of short scientific biographies, and a record of discoveries presented without logical coherence. What is one to make of a history of science that not only ignores all its branches save mathematics, astronomy, and physics in the early stages, but devotes more space to Virgil and Cyrano de Bergerac than to Aristotle and Galileo? Again, there are separate chapter headings for Rabelais and Montaigne—worthies indeed, but hardly major figures in the growth of science —while Vesalius is mentioned (with Servetus, and misleadingly at that) in 3½ lines. The medieval section in this book is poor, but the way in which the history of science in classical and early modern times is treated is hardly better.

The later sections are more useful, if only because more detailed—nearly half the volume is devoted to the present century. They have the virtue of making clear the difficulties that have to be overcome before a history, rather than a chronicle, of modern science can be written, difficulties that the author fails to surmount. It seems a pity that the vast labour of compilation devoted to this volume should have so little value as history. Has all the effort of the last fifty years to advance the study of the history of science in scholarship and intellectual standing been in vain? One cannot believe this: yet little of that effort is reflected in the structure and content of M. Perrin's history.

A. R. HALL

LIGHT WITHOUT HEAT

A History of Luminescence from the Earliest Times until 1900, *by E. Newton Harvey. Pp. xxiii* + 692. *The American Philosophical Society, Philadelphia.* 1957. $6 *net.*

Within the space of a brief review it is impossible to do justice to this remarkable book, where the history of luminescence from ancient China and Japan to the close of the nineteenth century is described with a wealth of references to original authorities: the select bibliography alone runs to 69 pages of small type. The study of luminescence has in fact been the life work of Professor Newton Harvey, primarily as regards the production of light by luminous organisms but not neglecting inorganic sources. Though the phenomena themselves have always exerted a peculiar fascination on the human mind, they are so varied that their investigation involves the application of many different branches of science, and it is not surprising that the origin of many natural weak lights remained a mystery until comparatively recent times.

In the present book the author offers first a general survey of the increasing knowledge of luminescences and then deals with the special types of luminescence associated with the non-living and the living world respectively. Among the non-living types are electroluminescence, phosphorescence, thermoluminescence, triboluminescence, fluorescence, radioluminescence, and chemiluminescence, while under the general heading of bioluminescence come the shining of fish, flesh, and wood, the phosphorescence of the sea, and animal luminescence. Here we find such curious information as that, in 1782, Russian midges became infected with bacteria that gave them a fatal disease that made them luminous; that Spallanzani crushed two large medusae in thirteen ounces of water until no more light appeared and so obtained 'an artificial phosphor' which shone when heated to 30° Réaumur; and that in 1790 decaying potatoes in a barracks at Strasbourg gave off enough light to read by.

Lest these examples convey the impression that the book is merely a collection of *curiosa*, it should be emphasized that the atmosphere throughout is strictly scientific, the section on bioluminescence—a subject to which Professor Newton Harvey has made many important original contributions—being particularly valuable. The folklore concerning the glow-worm and firefly is being prepared for a later volume—in which English readers will also hope to find an account of the natural history of 'The Dong with a Luminous Nose'!

E. J. HOLMYARD

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HISTORY OF CHEMISTRY

Through Alchemy to Chemistry, *by John Read. Pp. xvii + 206. G. Bell & Sons Ltd., London.* 1957. 18*s.* 6*d. net.*

To Professor Read 'Chemistry is the most romantic of all the branches of science'; it is the culmination of a long and variegated history. Undismayed by the magnitude of his task, he has taken as his subject the whole field of that history and he here presents it to the general reader within the confines of one small book. Much has had to be sacrificed in the process: but there has also been gain. He seeks to counter the over-specialized outlook of contemporary scientific research by stimulating interest in 'the broad humanistic aspects of science', and his survey of alchemical traditions shows that the gradual emergence of modern chemistry, as we know it today, is a positive and experimental expression of what had for centuries been a speculative pursuit of philosophers and craftsmen.

The chapters on the beginnings of alchemy in the ancient world are of especial interest. Professor Read is at his best when he discusses the mythological interpretation of alchemical ideas in the seventeenth century that centred around the person of Michael Maier, and the part played by the pseudo-alchemists. The latter is of prime importance in the matter of a just appraisal of the hundreds of alchemical texts.

There are a glossary and many reproductions of old illustrations, chosen with considerable care. G. HEYM

HISTORY OF TECHNOLOGY

A History of Technology, *Vol. III.* From the Renaissance to the Industrial Revolution, *c.*1500–*c.*1750, *edited by Charles Singer, E. J. Holmyard, A. R. Hall, and Trevor I. Williams. Pp. xxxvii + 766. Clarendon Press, Oxford.* 1957. £8 8*s. net.*

The third volume of this unique contribution to the history of technology is devoted mainly to the years between the Italian Renaissance and the Industrial Revolution, that is approximately 1500–1750. Like its predecessors, it contains a number of chapters dealing with the various branches of technology, each written by a specialist who can speak with authority. In spite of the compression needed to cover the whole field within a reasonable compass of 760 pages, each account is comprehensive, clear, and free from technicalities. The chapters are grouped under the headings of primary production, manufacture, material civilization, communications, and the approach to science. Between them they cover very completely the progress of technology during the centuries when the germs of the evolution of our modern scientific and industrial civilization were first becoming clearly visible.

Unlike the preceding volumes, this one deals exclusively with advances in the West, for the years in question were those that saw the rise of its dominance over the East, with the birth of modern science and of the machine age based on coal and iron. It was a fascinating age of progress, of great inventiveness, and of constant effort. Technology was changing the economic and social pattern of Europe, awaiting only the stimulus of the steam engine for the great industrial expansion of the West. Science was just beginning to contribute to some techniques, for instance navigation, but on the whole science owed more to technology than technology did to science. That had to wait until scientific theory had found a surer basis. The prime mover in this development was invention, stimulated by the growing needs of the Western nations with their restless energy that marked these centuries.

It is impossible in this short review to do justice to the range, the quality, and the interest of each chapter, in which a wealth of illustrations does much to bring their stories to life. Each reader will have his own preferences: I enjoyed particularly the chapters on printing by Michael Clapham, and on precision and scientific instruments by Derek Price.

Suffice it to say that this volume more than maintains the standard of the first two, thanks to the care and skill of the editors. One's only regret is that the high price of the book, in spite of the generous subscription by Imperial Chemical Industries, will inevitably limit the number of bookshelves on which it will find a most welcome and valuable place. HAROLD HARTLEY

HISTORY OF IRON AND STEEL

History of the British Iron and Steel Industry from *c.* 450 B.C. to A.D. 1775, *by H. R. Schubert. Pp. xxi + 445. Routledge and Kegan Paul, London.* 1957. 60*s. net.*

The high expectations raised by Dr Schubert's short notes in the 'Journal of the Iron and Steel Institute' are fully met by this more complete work. The author begins by analysing the archaeological evidence on the first production of iron in the British Isles. He then traces the subsequent regional distribution of the industry and changes in methods of working, from the primitive bowl furnace through the displacement of the direct process by the blast furnace and finery. He treats the introduction of coke very briefly, and his terminal date excludes consideration of the new epoch started by Cort's puddling process and the introduction of steam power. The book is well illustrated and has important appendices summarizing statistical data and giving *in extenso* the text of some seventeenth and eighteenth century records and descriptions.

The book will be particularly valuable to the economic historian, for the new documentary sources that the author has found in public and private archives indicating the magnitude of the industry at various places and times present a much more complete picture than was previously available. Such sources are less satisfactory for answering technical questions about processes. Dr Schubert's discovery of the unfinished manuscript of William Lewis's six-volume 'Chemical and Metallurgical History of Iron' is important, for this is the first treatise on iron written in English by a man of scientific stature. At best, however, the siderurgical literature of Britain is sparse, and one wishes that the nationalistic restriction had been relaxed to allow the author to make more extensive use of the richer Continental published material. Of course, such technical literature is often misleading, for literary ambitions are rarely joined with practical knowledge, and this is more than usually true with ferrous metallurgists.

The lack of such a background of English technical source material affects particularly the sections of the book dealing with steel.

Altogether, however, this is a fascinating story of iron and of the waves of technological improvement that have successively changed its nature and importance. There are few of man's activities that have involved so fine an interplay between a knowledge of the properties of materials and skill in the organization of men. Dr Schubert's nose for new sources of information and his ability to analyse and present it in an interesting way makes this a most important book. Both historians and metallurgists will be grateful to him, and to the Iron and Steel Institute whose support made the work possible. CYRIL S. SMITH

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Short notices of books

(These notices are descriptive rather than critical and are designed to give a general indication of the nature and scope of the books.)

Elements of Heat Transfer (*third edition*), *by M. Jakob and G. A. Hawkins. Pp. xxv + 317. John Wiley & Sons Inc., New York; Chapman and Hall Ltd., London.* 1957. 54*s. net.*

The two previous editions of this book were titled 'Elements of Heat Transfer and Insulation' and were students' textbooks of heat transfer. The original edition was deliberately confined to fundamentals; the present volume has continued the process, initiated by the second edition, of adding more difficult methods and details.

The chapter on thermal conductivity now includes a table on the thermal conductivity of liquid metals. Other additions include the use of equivalent circuits for the solution of heat transfer problems, and one type of thermal analyser. There are new sections added to the chapters on heat transfer by forced convection, and by combined conduction and convection. A new chapter on mass transfer has been added, and a section introducing the student to the important subject of gas radiation. The references have been brought up to date, and more problems are included.

The Proceedings of the Third International Conference on Electron Microscopy, held in London 1954, *edited by R. Ross. Pp. xvi + 705. Royal Microscopical Society, London.* 1956. 90*s. net.*

The 158 papers read at this important international conference most comprehensively cover the whole field of electron microscopy. They consider, for example, the design of instruments and the finished apparatus; the effect of electrons on the specimens; metallurgical applications; viruses and bacteria; interference and phase effects; internal structure of cells; replica methods; and the operation of electron microscopes. The whole collection forms a valuable work of reference.

The Planet Earth, *edited by D. R. Bates. Pp. 312. Pergamon Press Ltd., London.* 1957. 35*s. net.*

The intelligent layman is the intended reader of this introduction to the background of the International Geophysical Year. Geophysics is completely covered, and each chapter is written by an authority.

The subjects include the nature of the Earth, the oceans, and the atmosphere, the origins of the Earth and of life, and the possible fate of the Earth. There is a selected bibliography giving further reading for each section.

The Planning of International Meetings. A C.I.O.M.S. Handbook. *Pp. 113. Blackwell Scientific Publications, Oxford.* 1957. 7*s.* 6*d. net.*

UNESCO and WHO jointly established the Council for International Organizations of Medical Sciences, which was founded in 1949 and has aimed at raising the standard of international medical meetings and at coordinating congresses. The book discusses all types of conference, from the large international congress to the committee, and covers all aspects, from preliminary organization to publication of the journals and proceedings of the congress and its evolution. Separate sections deal with the solution of language problems, relation of the organizers with participants, and social events. Appendices give specimen application forms for visitors and participants and specimen guides to contributors.

Pioneering in Industrial Research. The Story of the General Electric Research Laboratory, *by K. Birr. Pp. vii + 204. Public Affairs Press, Washington, D.C.* 1957. $4.50 *net.*

Dr Birr's history of the General Electric Research Laboratory is a detailed study of the laboratory's organization, administration, and accomplishments.

It opens with a general history of industrial research, continues with a short outline of the early history of the companies that formed the General Electric Company, and then covers the history of the General Electric Research Laboratory from its foundation in 1901 under its first director, Willis R. Whitney. The research mentioned starts with work on the electric bulb, and the story is continued to the recent production of artificial diamonds. The book is by an historian and does not contain details of technical processes.

Ciba Foundation Colloquia on Endocrinology. *Vol. XI,* Hormones in Blood. *edited by G. E. W. Wolstenholme and Elaine C. P. Millar. Pp. vi + 416. J. and A. Churchill Ltd., London.* 1957. 56*s. net.*

The four-day colloquium 'Hormones in Blood' reported in this volume was organized by the Ciba Foundation in February 1957. All the papers emphasize the study of the hormones in the blood as opposed to their study when excreted in the urine. The twenty-one papers together with the discussion on them published here give a very complete view of the present state of knowledge of physiologists, biochemists, biologists, and endocrinologists in this field, and outline the trends of present-day research into the major problems.

Hides, Skins and Leather under the Microscope. *Pp. 368. The British Leather Manufacturers' Research Association, Egham, Surrey.* 1957. £8 10*s. net.*

After thirty-five years' work the British Leather Manufacturers' Research Association possesses over 30,000 photomicrographs, a collection unparalleled elsewhere. Nearly a thousand are reproduced in this book, illustrating the structure of a wide range of hides, finished leathers, damaged or diseased material, and so on. Each illustration has a detailed explanatory caption. This is a very comprehensive work of reference.

The Chemistry of Natural Products. *Vol. I.* The Alkaloids, *by K. W. Bentley. Pp. vii + 237. Interscience Publishers Inc., New York; Interscience Publishers Ltd., London.* 1957. $4 *net.*

The important facts about the structural chemistry of the more important alkaloids, and the degradation and synthetic means by which these structures were elucidated are covered by this book. It is directed at the undergraduate student with a view to extending the limited treatment of the subject in the general textbooks without attempting specialized detail. Alternate pages are wholly devoted to formulae with a view to making the subject more easily understandable.

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Notes on contributors

SIR GAVIN DE BEER,
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Was born in 1899 and educated at the *Ecole Pascal*, Paris, at Harrow School, and at Magdalen College, Oxford. He was a Prize Fellow of Merton College, Oxford, 1923–38, and Senior Demonstrator in Zoology, University of Oxford, 1926–38. In the second World War he served as a lieutenant-colonel in the Grenadier Guards in command of psychological warfare in 21 Army Group. He has been Director of the British Museum (Natural History) since 1950. He will be president of the 15th International Congress of Zoology, which will meet in London in July this year.

N. P. INGLIS,
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Was born in 1902 and was educated at the Liverpool Collegiate School and the Universities of Liverpool and Illinois. He joined Synthetic Ammonia and Nitrates Ltd. (now the Billingham Division of Imperial Chemical Industries Ltd.) in 1927 as a metallurgist, and in that capacity carried out research and development on metals suitable for what were then novel chemical processes, including in particular work on the influence on steels of hydrogen at elevated pressures and temperatures, and also on problems connected with the use of corrosion-resistant steels. He was appointed a director of the Metals Division of I.C.I. in 1947 and its Research Director in 1951. He is a vice-president of the Institute of Metals.

M. K. McQUILLAN,
M.A.,

Was born in 1921 and went to Cambridge in 1939. She joined the Metallurgy Division at the Royal Aircraft Establishment on graduation in 1942, and shortly afterwards became a member of a newly formed group established to work on materials suitable for jet-propelled aircraft. Her active interest in titanium dates from this time, and continued while she was at the Aeronautical Research Laboratories, Melbourne, and at the Metals Division of Imperial Chemical Industries Ltd. In 1946–47 she was one of a group from the Royal Aircraft Establishment which formed the nucleus of the Metallurgy Division at the Atomic Energy Research Establishment, Harwell. With her husband, Professor A. D. McQuillan, she has published 'Titanium', a book on the metallurgy of the metal.

H. S. W. MASSEY,
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Was born in 1908 and was educated at the University High School, Melbourne, and Melbourne University. After graduation he undertook research work in atomic physics at the Cavendish Laboratory, Cambridge. In 1933 he was appointed head of the department of mathematical physics at Queen's University, Belfast, and in 1938 became Goldsmid professor of mathematics at University College, London. He held this chair until 1950, when he became Quain professor and head of the department of physics at the same college. In addition to many papers and reviews in scientific journals he is joint author of two monographs, 'The Theory of Atomic Collisions' (with N. F. Mott) and 'Electronic and Ionic Impact Phenomena' (with E. H. S. Burhop); he has written three other books, 'Ancillary Mathematics', 'Atoms and Energy', and 'Negative Ions'.

W. O. JAMES,
M.A., D.Phil., F.R.S.,

Was born in London in 1900 and was educated at Tottenham Grammar School and the Universities of Reading and Cambridge. He worked first at Rothamsted on the potassium nutrition of plants. He is Reader in Botany in the University of Oxford, and since 1927 has led a school of teaching and research in plant physiology, especially concerned with nutrition and respiration. During the war and after he was Director of the Oxford Medicinal Plants Scheme, dealing with the production of drug plants in Britain and with the study of alkaloid metabolism in the *Solanaceae*. He is an editor of the 'New Phytologist', and has published 'The Biology of Flowers' (with A. R. Clapham), 'Plant Respiration', some well-known textbooks, and numerous papers in several branches of botany.

ABDUS SALAM,
M.A., Ph.D., D.Sc.,

Was educated at the Universities of the Punjab and Cambridge. He was elected to a research fellowship of St. John's College, Cambridge, in 1951 and was appointed Professor of Mathematics at Government College, Lahore, in the same year. He became Head of the Department of Mathematics in the University of the Punjab in 1952 and a lecturer in the University of Cambridge in 1954. In 1955 he was Scientific Secretary at the Geneva conference on the peaceful uses of atomic energy. He has been Professor of Applied Mathematics at Imperial College, London, since 1957. His work has been in theoretical physics, particularly in the development of quantum electrodynamics and of theories interpreting the properties of mesons and other recently discovered fundamental particles.

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Internationalism in science

The International Geophysical Year that began in July 1957, after more than five years preparation, is the greatest single programme of scientific co-operation ever attempted. Its present success is a welcome reminder of what far-reaching scientific results can be achieved by friendly co-operation between nations, but nevertheless it cannot fail to provoke thoughts of fields in which this spirit of harmony is noticeably lacking.

Scientists of the present generation were educated in the belief, which still persists, that science knows no frontiers, and that if its material rewards are modest, at least it is above the international strife which bitterly divides men in so many other activities. History could provide some comforting illustrations to support this belief. Rumford was honourably received in Paris even when France and Britain were in the throes of bitter war; Benjamin Franklin, during the War of American Independence, gave special instructions that Captain Cook was not to be molested by American cruisers while on his voyages of scientific exploration. It is true that in very many fields national frontiers are still unimportant: collaboration is limited only by the initiative and resources of individuals, and there are no restrictions upon publication. The areas of restriction may be relatively small, having regard to science as a whole, but they are of exceptional significance and represent very great expenditure in both money and manpower.

We have here a strange paradox. On the one hand, there is general agreement that many of the great material problems of the world will not be solved without the widespread application of new scientific and technological discoveries. On the other, we see a vast and unnecessary duplication of effort, especially in the fields that promise to be the most rewarding, at a time when even the most far-sighted nations have a great shortage of scientists and technologists. The political reasons for this paradox are familiar enough. As long as there is deep distrust between nations and groups of nations, none, very obviously, will risk sharing with others secrets upon which survival might depend. Even between firm allies the free exchange of knowledge is manifestly difficult, for the tide of war might well cut off one partner from another, and in consequence every major nation feels an urge to be as self-sufficient as possible.

Distrust between nations is as old as civilization, and it would be naive indeed to suppose that the intensely difficult political problems of today will wilt away if we remove artificial barriers to the onward march of science. Nevertheless, science is undeniably the biggest single factor determining the future of mankind, and it is of the first importance that the fullest possible use be made of the possibilities for material advancement that it offers. This clearly cannot be done while progress in some of the most potentially fruitful fields is artificially restricted, as it is at present.

The development of thermonuclear power is a topical and striking example. Here we have a project that seems destined ultimately to provide the world with cheaper and more abundant power than could have been dreamed of even twenty years ago. The possible benefits to mankind as a whole are incalculable, and if ever there was a field in which one would wish science to transcend national frontiers this is surely it. Yet it has not done so. The publication of scientific results of the first importance has been delayed for political reasons, and co-operation between the various national research centres has been at best belated and incomplete, at worst non-existent. There are always those who can advance good reasons why an obviously unsatisfactory state of affairs is best left unaltered, and in the absence of counter-argument they will carry the uninformed with them, but the present situation is one that all scientists must deplore and seek to change.

In present circumstances the scientist has a particular interest and duty in the achievement of international relations cordial enough for all his resources to be deployed for peaceful ends. Like every other person, he has an acute personal interest in surviving in peace rather than perishing in war. But beyond this, his special knowledge encourages him to see the dawning of a golden age, such as the world has never known, in which the fruits of science, wisely applied, can be increasingly used for the benefit of mankind. Science alone can solve few problems, but, given opportunity and the co-operation of others upon whom our complex civilization depends, it can certainly remove many of the material causes of human unhappiness. Science as a dominant force in civilization is very new—which in itself is doubtless an important reason why its achievements so far outstrip general recognition of both its possibilities and its dangers—but its beginnings can be traced back for many centuries. A world in which man's

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mastery of nature is beneficently applied seems almost as far away now as when it was adumbrated by such far-sighted men as Francis Bacon in the seventeenth century. It is tantalizing that a prize so rich and so long sought cannot be enjoyed to the full, and may even be lost again for generations, because of bitter human differences.

Many scientists see clearly enough how much might in favourable circumstances be achieved by the full exploitation of our present knowledge, let alone of the knowledge that may reasonably be expected to accrue in the next few decades. But are these possibilities clear to those who shape national policies and, above all, to those who elect to office those who shape such policies? Unhappily, they are not. A great part of the world still fears science and sees in it not a benefactor but a destroyer: this is scarcely surprising when one of the greatest scientific discoveries of all time has been primarily exploited as a threat between nations.

But if politicians have been, and still are, slow to see the full implications of new discoveries, scientists themselves are not blameless, for at a time when they should have given a positive lead they have too often been inactive. Though organizers of the greatest single material force in the world today they have as a group done little to inspire the public with the truly great possibilities arising from it. It is true that the popular exposition of science has improved enormously during the past decade, both in quality and in quantity, but the initiative has in the main come from outside science and not from within, and still does so. Scientific men are usually ready to give advice when it is called for in a good cause, but how few have exerted themselves to make scientific ideas known where they would never have been sought and to propagate their own faith in science. Ability to spread new ideas among those who see no particular need for change is unfortunately not necessarily acquired at the same time as scientific education and experience. It demands personal qualities that are doubtless as rare among scientists as they are among the population at large, but they certainly exist, and should be much more widely exercised than they are. The main explanation is doubtless that the absorbing interest of science tends to make those devoted to it reluctant to leave it for other fields.

Industry has long realized the importance of good public relations, and its policy of actively promoting these has unquestionably been a powerful factor in its recent great development. In science the position is altogether different; any sort of public relations organization is looked upon by some scientists as not only unimportant but even as in some way unbecoming. This attitude is gradually changing, more rapidly in some countries than in others, but the few agencies that now exist for the primary purpose of disseminating scientific information to the public too often labour not merely under financial difficulties but even without the degree of support they are entitled to expect from scientists themselves.

Replacement of the fear of science by realization that if properly used it offers tremendous benefits for mankind is of the utmost importance, not least to science itself. It is now not merely possible but important to go further than promises, and to provide tangible evidence of what can be done if scientists from all nations unite upon some great practical project, as they are at present doing in the I.G.Y. One such project has lately been proposed: it is for the eradication of malaria. This is a disease believed to affect some 700 million people and to cause perhaps 10 million deaths annually. It is also a disease which existing knowledge—if deployed with sufficient force behind it—could eradicate as surely as vaccination and public health measures have virtually abolished such diseases as diphtheria and cholera from many countries where they were once rife.

Such a project, or any other that would tax the combined resources of all to the utmost, deserves the fullest support. It could both capture the imagination of the public and convincingly demonstrate that the applications of science are not a matter of chance—when evil and good results have equal chances of emerging—but can be deliberately steered into beneficent channels if mankind so wills. It would, moreover, be a major contribution to world peace. While the underlying causes of war so far defy analysis, one important factor undoubtedly is that man's restlessness demands an outlet. War is one such outlet: vast international projects such as science has made both possible and profitable might well be an effective alternative.

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Thermonuclear power and the pinch effect

J. E. ALLEN

At present power-producing nuclear reactors utilize the fission process. A possible alternative, however, is offered by the fusion of hydrogen isotopes. Some discussion of the fusion process is given, and it is shown that fusion power will probably be obtained from a thermonuclear reaction if it can be obtained at all. The extreme physical conditions that must be developed in a power-producing reactor are discussed, and the self-magnetic pinch effect, which offers one of the more promising methods of achieving these conditions, is described in some detail. The instabilities that arise in pinched discharges are described, together with the means used to combat them, and an outline is given of methods that can be used to deliver energy to a pinched discharge, together with possible ways of extracting energy.

In a recent issue of ENDEAVOUR Sir Christopher Hinton [1] described the nuclear reactors which are now being built to produce electrical power. These utilize the fission reaction that takes place among heavy elements. In this reaction a heavy nucleus is split into fragments, with an accompanying release of nuclear energy. Another way of obtaining nuclear energy, however, is by the fusion of light elements. In both these reactions the mass of the products is less than the mass of the original materials, and the energy produced is the equivalent of the matter that has vanished. The latter process is operative at the centre of the Sun, where hydrogen atoms are fused together to form helium, and also in the hydrogen bomb. It has been estimated that the conversion rate at the centre of the Sun is about 800 million tons of hydrogen per second.

The probability of a fusion reaction taking place, as opposed to the more usual elastic collision, is known, and it is found that by far the most probable fusion reactions are those involving the hydrogen isotopes deuterium and tritium (hydrogen 2 and hydrogen 3 respectively). Of these, deuterium is plentiful on the Earth, particularly when compared with the fissile isotope of uranium, since it forms about one part in 5000 of natural hydrogen. Thus the oceans contain a virtually inexhaustible supply of fuel if an efficient way of 'burning' deuterium can be found. Tritium, on the other hand, does not occur naturally.

In addition, if deuterium can be used as a fuel, no appreciable amount of radioactive waste is likely to be produced. This would represent a great advantage over fission power, where the disposal of radioactive waste will become increasingly difficult. It is considerations such as these, together with the intrinsic appeal of the problem, that have aroused world-wide interest in the possibility of using the fusion reaction as a source of power.

THE FUSION REACTION

Protons, the nuclei of ordinary hydrogen, have a low probability of fusing in an encounter, but the nuclei of deuterium and tritium are much more likely to fuse. The following reactions may take place in a collision between two deuterons:

$$H^2 + H^2 \rightarrow He^3 + \text{neutron} + 3{\cdot}3\ \text{MeV} \quad \ldots.(1)$$

$$H^2 + H^2 \rightarrow H^3 + H^1 + 4\ \text{MeV} \quad \ldots.(2)$$

If a deuteron collides with a triton (the nucleus of a tritium atom) they may undergo the reaction

$$H^2 + H^3 \rightarrow He^4 + \text{neutron} + 17{\cdot}6\ \text{MeV} \ldots.(3)$$

The first two reactions are equally probable; the third is of interest because it is easier to achieve, and also because it involves a reaction product of the second reaction. Thus if the tritium formed in reaction 2 is 'burnt' as soon as it is formed, 24·9 MeV is obtained from the reaction of five deuterons.

Figure 1 shows graphs of the 'cross sections' for the fusion reactions between two deuterium nuclei, and between a deuterium nucleus and one of tritium. The cross section can be thought of as the target area which must be hit if the reaction is to take place, and is a measure of the probability of the reaction. The deuterium-tritium (D-T) reaction has a greater cross section than the fusion reaction of two deuterium nuclei (called a D-D reaction), but tritium, as mentioned already, does not occur naturally. It can be 'bred' by capturing

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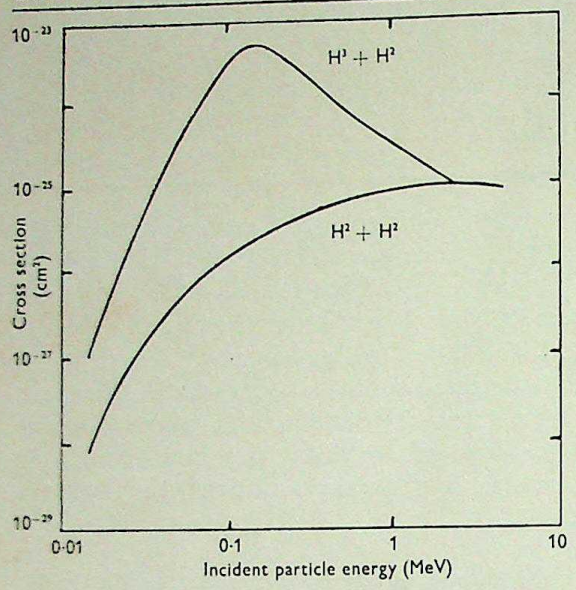

FIGURE 1 – *Cross sections for D-D and D-T reactions. The lower curve refers to either of the D-D reactions, which have an almost identical cross section.*

neutrons (from the D-D reaction) in the light isotope of lithium, Li^6,

$$Li^6 + \text{neutron} \rightarrow He^4 + H^3 + 4{\cdot}8 \text{ MeV}$$

but lithium is much less abundant than deuterium. It is, therefore, highly desirable to use the D-D reaction for energy production if possible.

The most probable consequence of an encounter between two nuclei is an elastic collision, due to the Coulomb repulsion between the two charged particles. This is true even at energies up to 0·1 MeV, so that there will be a large number of elastic collisions for a small number of nuclear reactions and the velocities of the nuclei will quickly become 'randomized'. This strongly suggests that efficient fusion power will be obtained from 'thermonuclear' reactions, that is, reactions in which the nuclei have a thermal distribution of velocities, similar to that possessed by the molecules of an ordinary gas.

Whether or not two colliding particles fuse depends on their relative velocity. In a hot, completely ionized gas the nuclear reactions take place in collisions between the most energetic particles. W. B. Thompson has shown [3] that the reaction rate can be written in the form $r = n_a n_b \langle \sigma v \rangle$ for reactions between unlike particles and $r = \frac{1}{2} n^2 \langle \sigma v \rangle$ for reactions between like particles. Here n_a, n_b, and n are particle densities (number/cm³), and $\langle \sigma v \rangle$ is the product of the cross section and the relative velocity averaged over the Maxwellian distribution of velocities. The reaction probability $\langle \sigma v \rangle$ depends only on the temperature, and figure 2 shows the curves computed by Thompson for the D-D and D-T reactions. Thompson also considered a number of other reactions, but concluded that the D-D and D-T reactions were the only significant ones for temperatures less than 10^9 °K.

In the case of the D-D reaction (taking both reactions (1) and (2) together) 66 per cent of the reaction energy is given to the charged reaction products, on the average, and 34 per cent is given to the neutrons. In the case of the D-T reaction only 20 per cent of the reaction energy is given to the charged products and 80 per cent to the neutrons. These figures are of interest, since the charged products might remain in the plasma but the neutrons will always escape and deposit their energy elsewhere, and this distribution of energy is one of the factors in the calculation of the conditions under which a thermonuclear reactor will produce more energy than it consumes.

REQUIREMENTS FOR A POWER-PRODUCING REACTOR

It is not known at present what form a thermonuclear reactor will take, or indeed whether one can be built at all. Nevertheless, it can be shown that temperatures considerably greater than 3×10^7 °K must be achieved if a reactor is to become a practicable proposition. At such temperatures the hydrogen isotopes are completely ionized, and the problem resolves itself into a study of the physical properties of a completely ionized gas. Such a gas

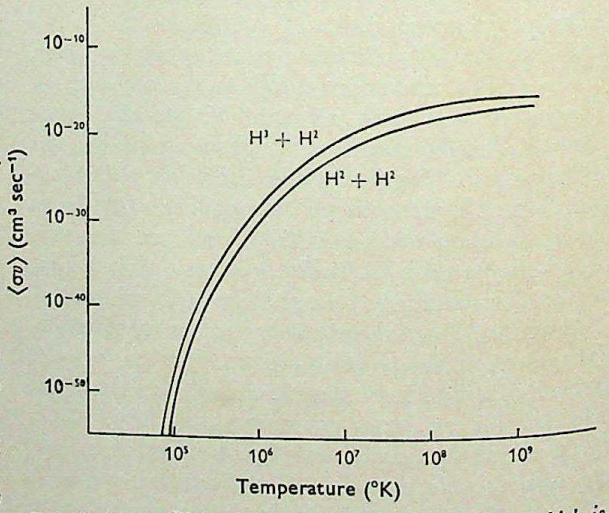

FIGURE 2 – *Plot of the reaction coefficient $\langle \sigma v \rangle$ which is used in the calculation of thermonuclear reaction rates. The lower curve refers to the combined D-D reaction.*

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differs from an ordinary one in that it interacts with electric and magnetic fields. In fact this interaction offers a possible solution to the problem, since one might be able to hold the gas away from the walls of its containing vessel by electromagnetic forces, so that conduction losses are negligible, and then heat it to literally astronomical temperatures.

An ionized gas has been referred to as a 'fourth state of matter' to emphasize its peculiar properties: in this paper it will be referred to as the 'plasma', following the nomenclature adopted by I. Langmuir and L. Tonks [2] in their pioneer work on the subject. A plasma in which most of the atoms are stripped of their electrons can emit little energy as line spectra, but a continuous spectrum known as 'bremsstrahlung' is emitted. A free electron has a finite probability of making a spontaneous transition from one hyperbolic orbit round a nucleus to another hyperbolic orbit of less energy, with the emission of a photon of frequency ν, where $h\nu$ is the energy difference between the two orbits. Bremsstrahlung radiation consists of the photons emitted in these 'free-free' transitions. Most of these photons escape from the gas without absorption, and they constitute an unavoidable energy loss. G. Cillié [4] has calculated the power emitted in this way from nebulae. His formula is applicable here, and reads

$$p = 1{\cdot}42 \times 10^{-34} n_1 n_2 \mathcal{Z}^2 T_1^{\frac{1}{2}} \text{ watts/cm}^3$$

where n_1 and n_2 are the electron and ion densities respectively, $\mathcal{Z}$ is the ionic charge, and T_1 is the temperature of the electrons (which are assumed to have a Maxwellian energy distribution). Provided the ion densities are not too small, n_1 is nearly equal to $\mathcal{Z}n_2$ throughout the greater part of any ionized gas (i.e. the plasma as a whole is neutral), so that the expression for the power loss can be written in the form

$$p = 1{\cdot}42 \times 10^{-34} n_2^2 \mathcal{Z}^3 T_1^{\frac{1}{2}} \text{ watts/cm}^3$$

The radiation differs from that of a black body because the conditions of thermodynamic equilibrium are not approached. In the latter case every process occurs at the same rate as its converse (this is the principle of detailed balancing), whereas in the present case the absorption is negligible compared with the emission, because the amount of matter is small. The factor $\mathcal{Z}^3$ in the equation shows that the radiation loss is more serious for highly charged nuclei, and for this reason only the elements of low atomic number are suitable as fuel for the thermonuclear reactor.

By comparing this formula with the figures for the thermonuclear yield J. D. Lawson [5] has shown that the radiated power is equal to the latter at a temperature of about $1{\cdot}5 \times 10^8$ °K for the D-D reaction and 3×10^7 °K for the D-T reaction. For comparison, the temperature at the centre of the Sun is about $1{\cdot}5 \times 10^7$ °K. In the case of the D-D reaction it was assumed that the tritium was burnt as soon as it was formed. These figures give some idea of the temperatures required, but the economical operation of a reactor demands even higher temperatures.

If the reaction products escape from the plasma and deposit their energy as heat which is then transformed into electrical energy by conventional means (as shown in figure 3*a*), the nuclear energy released must be at least twice the radiation loss, because the efficiency of conversion is only about 33 per cent. The lowest operating temperatures, calculated on this basis, are 2×10^8 °K for the D-D reaction and 3×10^7 °K for the D-T reaction. If the reactor operates in pulses, as seems likely, an appreciable amount of energy is required to heat the plasma to the appropriate temperature at the beginning of each pulse. For economical operation the electrical energy that is generated must exceed the sum of the radiated energy and the energy required to heat the plasma. Ignoring the dissociation and ionization energies and the losses during the heating time, Lawson showed that on this basis nt must be greater than 10^{16} for the D-D reaction, where n is the ion density (number/cm^3) and t is the time in seconds during which the plasma is hot. Thus if $n = 10^{16}$ deuterons/cm^3, t must be greater than 1 second. It is evident that long times and high ion densities are required in addition to very high temperatures. In the case of the D-T reaction conditions are easier, but still severe, and nt must exceed 10^{14}.

It is possible that the charged reaction products will remain in the plasma (as illustrated in figure 3*b*), since they are affected by electric and magnetic fields, but the neutrons will always escape and release their energy as heat when absorbed. Lawson states that the nt values are almost unaltered if the charged reaction products are contained.

At a certain temperature (3×10^8 °K for D-D and 5×10^7 °K for D-T) the nuclear energy associated with the charged reaction products is equal to the radiation loss. There is a possibility that, at higher temperatures than this, part of the nuclear yield may be fed directly into the electrical supply. The mechanism of this energy transfer will be discussed further in a later paragraph.

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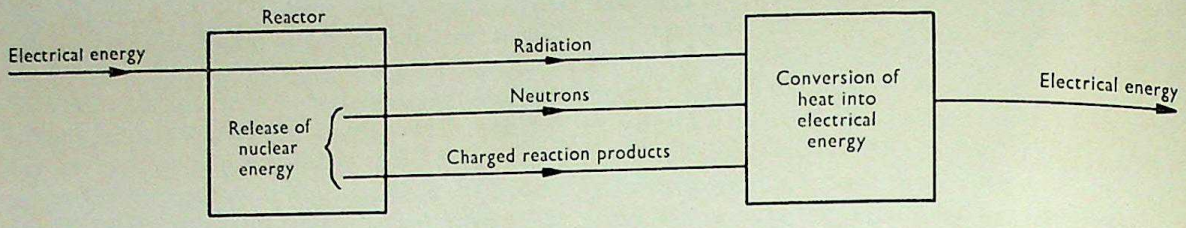
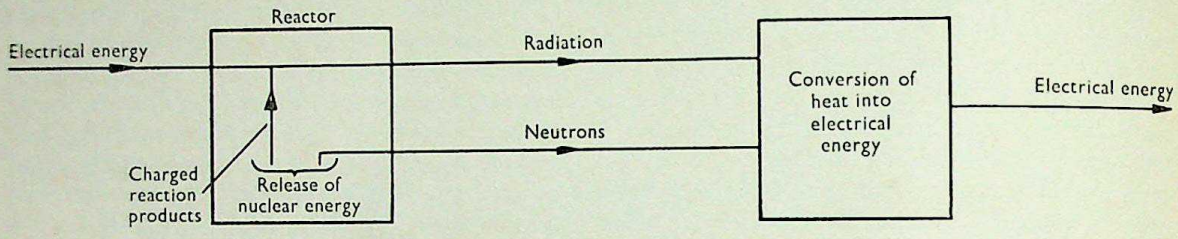


Case (*a*). *System in which the charged reaction products escape.*



Case (*b*). *System in which the charged reaction products are contained.*

FIGURE 3 – *The generation of electrical energy from a controlled thermonuclear reaction. During certain cycles of operation the electrical energy in case* (*b*) *may flow from the reactor to the electrical supply.*

These, then, are the ion densities and temperatures involved. One of the more promising methods of confining a plasma can now be considered.

THE PINCH EFFECT

A current-carrying conductor tends to be compressed under the action of its own magnetic field, as illustrated in figure 4. This is known as the pinch effect. It is due to the interaction of the current and the field, which produces an inwardly directed radial force, following Fleming's left-hand rule. Experiments on this effect, using liquid mercury, were reported by E. F. Northrup in 1907 [6]. The theory of the effect in an ionized gas has been developed by W. H. Bennett [7], L. Tonks [8], and others. In 1937, P. L. Bellaschi and R. C. Mason [9] stated that self-magnetic effects were present in the high-current atmospheric discharges which they were studying in the laboratory, and this type of discharge has been further studied by later workers [10–12].

From the thermonuclear point of view it is desirable to ionize the gas within a low-pressure discharge tube, and then to pinch it inwards, away from the walls of the tube. This effect was demonstrated in 1951 by S. W. Cousins and A. A. Ware [13], who were working under the direction of Sir George Thomson.

It can be shown that if a steady state is attained, wherein the pressure of the plasma is balanced by magnetic forces, then

$$I^2 = 4NkT \qquad \ldots.(4)$$

where I is the discharge current in e.m.u., N is the number of ions per unit length, k is Boltzmann's constant, and T is the temperature of the electrons or ions (assumed equal). It should be mentioned that although the magnetic force acts primarily on the electrons, since they carry the current, the electrons and ions are closely bound together by space-charge forces, as any appreciable separation of the electrons and ions would produce enormous

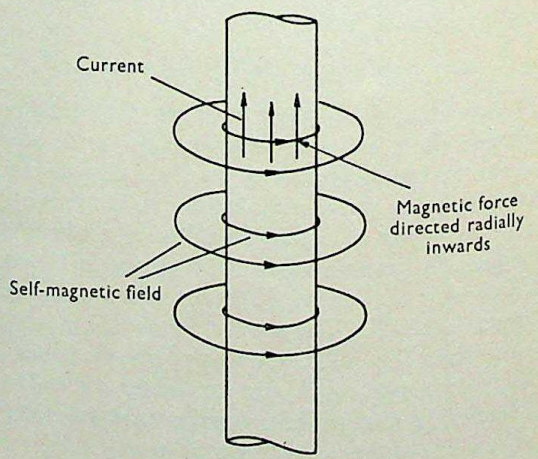


FIGURE 4 – *The self-magnetic pinch effect.*

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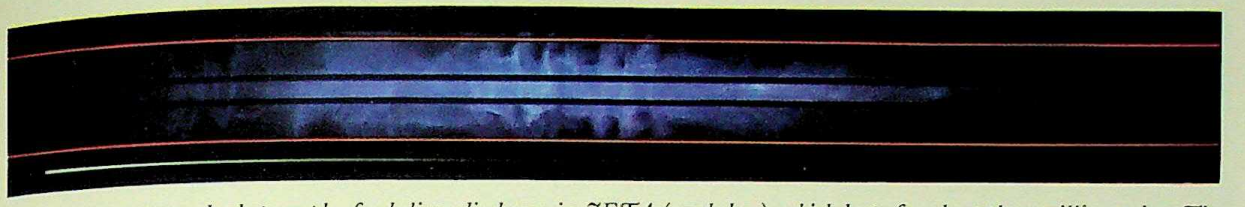

FIGURE 5 – *A streak photograph of a helium discharge in ZETA (see below) which lasts for about three milliseconds. The photograph was obtained while rapidly moving the film during the exposure, thus providing time resolution. The left-hand end of the lowest line marks the beginning of the pulse and the two fine lines indicate the diameter of the discharge tube (the thick black lines are due to obstructions blocking the window).*

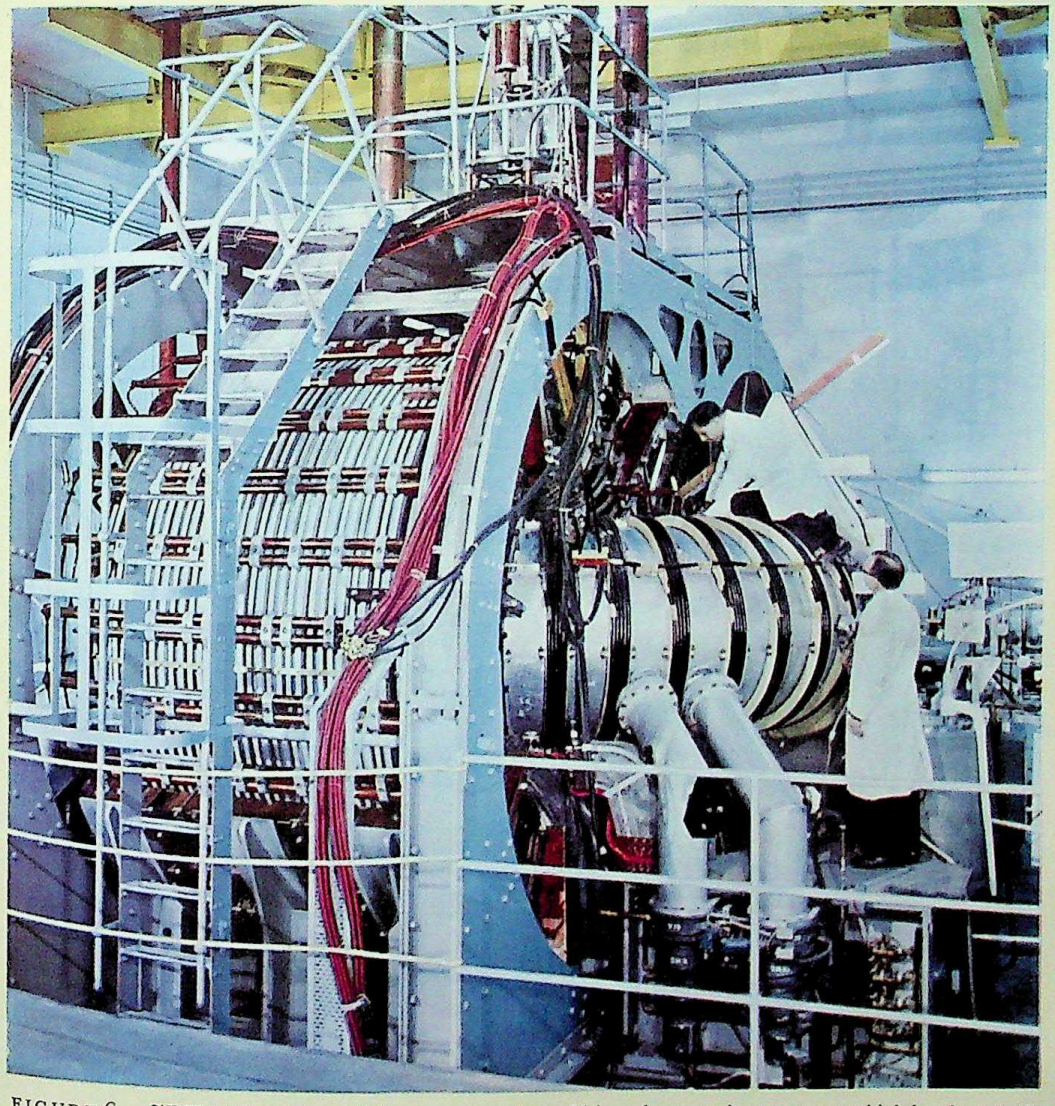

FIGURE 6 – *ZETA (Zero Energy Thermonuclear Assembly), a large-scale apparatus which has been built at the Atomic Energy Research Establishment, Harwell.*

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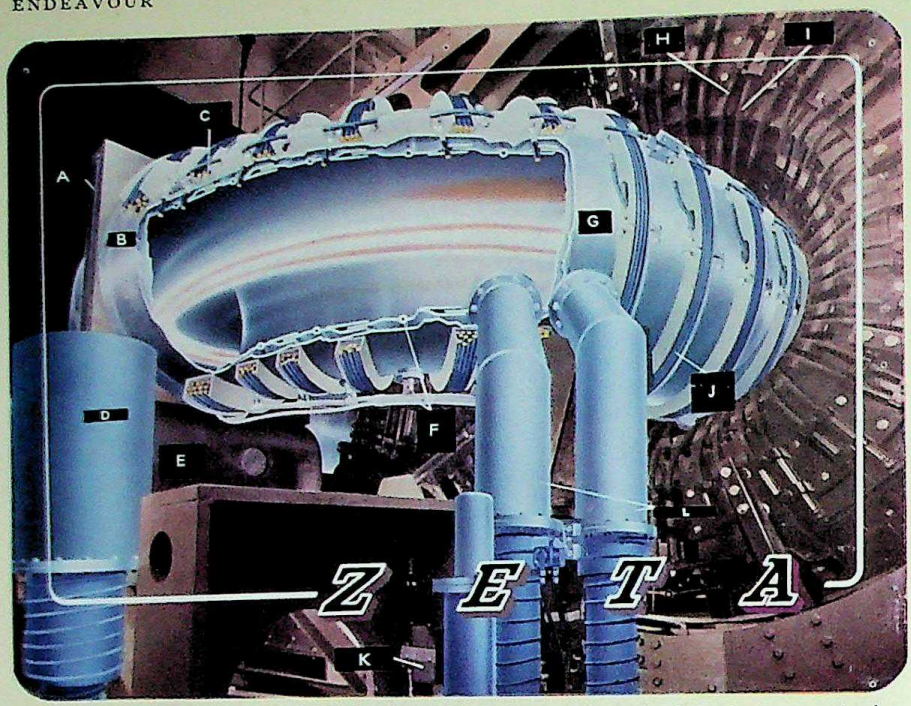

FIGURE 7 – *Cut-away view of ZETA, in which the letters refer to the following parts. A, viewing window; B, liners (of which there are 48), each insulated from its neighbours by insulators of polytetrafluorethylene C; D, liquid-air trap for the vacuum system; E, vacuum pumping manifold; F, water-cooling pipe for liner; G, torus body; H, primary windings of the transformer; I, bias windings; J, axial field windings; K, water-flow relay; L, vacuum pumps.*

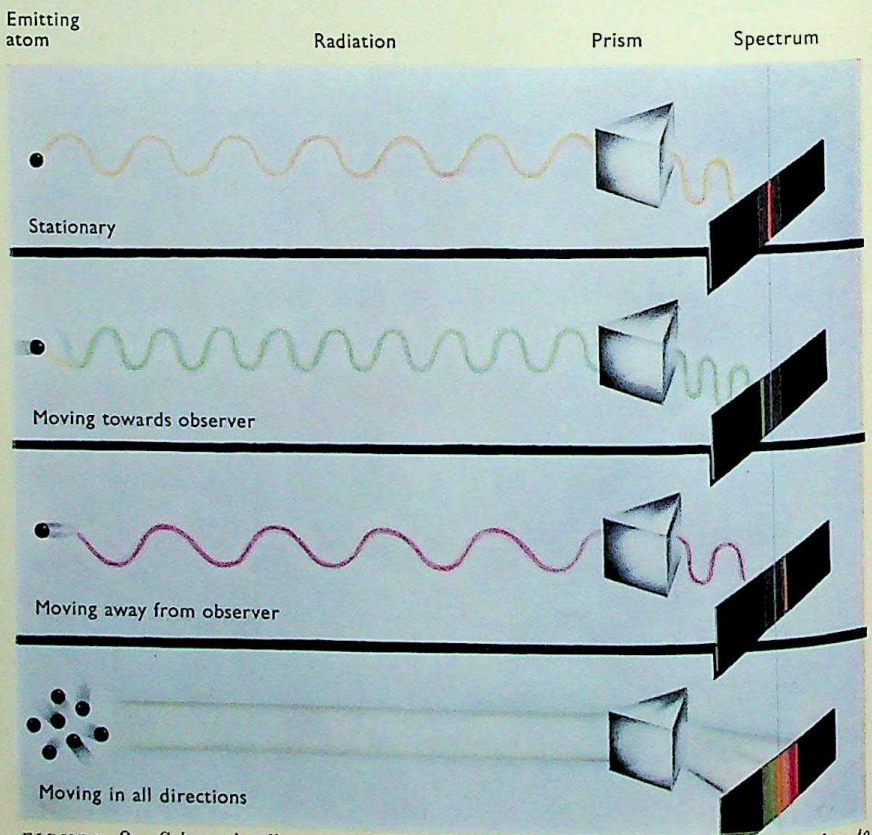

FIGURE 8 – *Schematic diagram illustrating the broadening of a spectral line due to thermal motion.*

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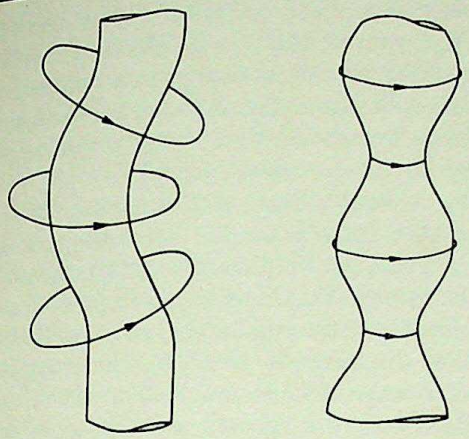

FIGURE 9 – *Instabilities of the pinched discharge.*

electric fields. A very small separation does occur, and the associated electrostatic field holds in the positive ions.

Heat losses to electrodes can be avoided by inducing the current in a ring-shaped discharge tube. The induced electric field is provided by linking the ring with a changing magnetic flux. Such a device is of course a transformer in which the plasma forms a single-turn 'secondary winding'. The transformer may or may not be designed with an iron core.

The main obstacle that has been encountered in experimental work on the pinch effect is the fact that the system is inherently unstable, as was shown by S. Lundquist [14]. Two possible modes of instability are illustrated in figure 9. These are usually referred to as 'kink' and 'sausage' instabilities respectively. In both cases the magnetic field strength is increased at certain points and decreased at others, and the resultant magnetic force tends to increase the deformation.

These instabilities and others have been discussed theoretically by M. D. Kruskal and M. Schwarzschild [15], and have been encountered in the laboratory by G. G. Timofeeva and V. L. Granovskii [16], I. V. Kurchatov and his colleagues [17], and others [18–20]. In general a pinched discharge will collapse—that is become constricted, as desired—and then undergo some radial oscillation, finally becoming unstable. Reproducible measurements can be made during the collapse time, which is determined essentially by the inertia of the ionized gas [17, 21, 22].

Instabilities are most undesirable, since conduction losses become appreciable if the plasma strikes the wall of the containing vessel. In addition, impurities are driven off the wall into the discharge, and these greatly increase the radiation

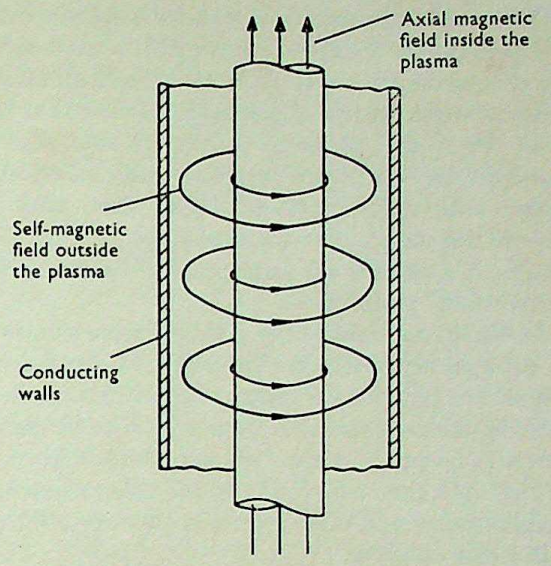

FIGURE 10 – *The stabilized pinched discharge.*

loss, and also increase the value of N in equation (4), thus lowering the temperature T. For these reasons it is important to consider how to stabilize the discharge.

CONTROL OF INSTABILITIES

The stabilizing effect of an additional magnetic field in the axial direction was first pointed out by Lundquist [14]. More recently the subject has been treated in greater detail by V. D. Shafranov [23], M. Rosenbluth [24], and R. J. Tayler [25]. These investigators have discovered a theoretically stable configuration which is illustrated in figure 10. The stability is achieved by including an axial magnetic field inside the plasma, restricting the self-magnetic field to the region outside the plasma, and by the presence of conducting walls. In this case the effective tension of the axial magnetic field is operative, which can be shown as follows. The magnetic field lines in a perfect conductor (the model used in these calculations) are 'frozen' into the conductor, since any relative motion between the field lines and the conductor generates an electric field in the latter, and this is contrary to the assumption of a perfect conductor. Thus if the plasma is deformed, so are the axial lines of force, and the tension tends to restore the plasma to its original position. Experimental work on this method was first reported by A. L. Bezbatchenko, I. N. Golovin, D. P. Ivanov, V. D. Kirillov, and N. A. Yavlinsky [26], who reported a delay in the onset of instabilities when the additional magnetic field was employed.

The sausage type of instability can be overcome

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by this tension but not the kink instability shown in figure 9, which can be suppressed, however, by the combined action of an internal field and conducting walls, provided the walls are not too far away from the plasma. If the plasma moves towards the wall, currents are induced in the latter, and, following Lenz's law, these tend to oppose the motion to which they are due, i.e. the magnetic force is such as to restore the discharge to its initial position.

In these calculations the currents were assumed to flow in an infinitely thin 'skin'. Calculations which are being made at present will show what skin depths are allowable if stability is to be maintained. In practice the two magnetic fields will diffuse into each other, due to the finite electrical conductivity, and after a certain time instabilities will begin to grow.

Once started, the instabilities grow rapidly, the time involved being comparable with the transit time for a sound wave travelling across the plasma.

THE ORTHOGONAL PINCH EFFECT

While no attempt is made here to discuss plasma containment from a general point of view, one other case may be mentioned—the 'orthogonal' pinch effect. In this the current and magnetic field shown in figure 4 are both rotated through 90°. The magnetic lines of force become straight, the lines of current flow become circles, and the resulting magnetic force is still directed radially inwards. An advantage of this system is that it is more stable than that producing the ordinary pinch effect, but it suffers from the disadvantage that it cannot be bent round to form a ring, as field variations would then destroy the equilibrium.

The circulating current must be induced in the plasma by changing the current in an external coil. For this reason the method is often referred to as compression by an external current. This is somewhat misleading, however, since currents must flow both in the plasma and in external conductors in either 'pinch-effect' system.

THE HEATING PROBLEM

The gas must not only be held away from the walls of the containing vessel, but it must be heated to a very high temperature. The energy can be supplied either by Joule (resistance) heating or by compression.

The energy associated with Joule heating is given primarily to the electrons, which carry the current. In due course the energy is shared with the positive ions in collision processes. The electrons must make many collisions before the ions are heated, however, because an electron can lose only a small fraction of its energy in an encounter with a massive positive ion. At high temperatures the necessary 'relaxation time' may be quite long, depending on the ion density [27].

Partly because this heating time is long, there is a considerable interest in the mechanical compression of a plasma. This is effected by the magnetic pinch force as the discharge collapses. It can easily be shown that the mechanical power delivered to a pinched discharge is $\frac{1}{2}I^2dL/dt$, where dL/dt is the rate of change of inductance of the discharge. In certain cases this power may exceed that associated with Joule heating.

The behaviour of a plasma when subjected to a compressive force depends on (*a*) whether the compression is fast or slow (compared with sound transit times across the plasma), (*b*) whether an internal magnetic field exists, and (*c*) whether collisions take place between particles. Although it might be argued that collisions must take place if the gas is to be heated, i.e. if the particles are to be given the random motion associated with a high temperature, these collisions might take place after the compression of the discharge.

If collisions take place frequently the plasma may be compressed adiabatically or shock-heated, depending on whether the compression is slow or rapid. If collisions are rare and the gas is slowly compressed, the particles are accelerated perpendicularly to the internal field. The magnetic flux threading any particle orbit increases, and there is an induced electric field (according to Faraday's law of induction) which accelerates the particle round its orbit. The net result can be thought of as a sort of adiabatic compression in which only two degrees of freedom are involved. It can be shown that the energy of the particles is directly proportional to their density (number per cm^3) and that the latter varies directly as the magnetic field strength. If, on the other hand, collisions are rare and the compression is fast, strong collision-free hydromagnetic waves are generated. These form another subject for research at the present time [28].

It is conceivable that a plasma might receive enough energy in a single compression to raise it to a very high temperature. In certain cases, however, repeated compression and expansion will continuously heat the gas. One such case has been described by A. Schlüter [29], who discussed the situation in which the collision frequency is comparable with the frequency of compression.

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DIRECT CONVERSION OF NUCLEAR ENERGY INTO ELECTRICAL ENERGY

The electromechanical power that is supplied to a collapsing discharge is $\frac{1}{2}I^2dL/dt$; the mechanism can be thought of as an 'electric motor' action in which electrical energy is transformed into mechanical energy. By reversing the sign of dL/dt, i.e. by allowing the discharge to expand, the electromechanical power becomes negative, which means that energy is flowing from the discharge to the electrical supply. The 'motor' action is thus replaced by a 'dynamo' action.

If a thermonuclear reaction takes place in a pinched discharge and the charged reaction products are contained, as shown in figure 3*b*, an expansion of the plasma will result in a direct transfer of nuclear energy to the electrical supply, provided that the radiation loss is not too great. The temperature must therefore exceed 3×10^8 °K in the case of the D-D reaction or 5×10^7 °K in the case of D-T reaction, since only at these temperatures does the nuclear power associated with the charged reaction products exceed the radiation loss.

The nuclear energy which is imparted to the neutrons, on the other hand, can only be abstracted as heat. Thus, even if direct conversion is used in the future, it will almost certainly be used in conjunction with conventional generating equipment so as to use this neutron energy.

PRELIMINARY RESULTS FROM ZETA

The results that have been obtained with ZETA (Zero Energy Thermonuclear Assembly) have recently been reported [30]. This is a large experimental device which has been built at the Atomic Energy Research Establishment, Harwell (figure 6). (The phrase 'Zero Energy' is used because the energy produced in the fusion reaction is negligible compared with the energy input.) The discharge tube is an aluminium torus of 1 metre bore and 3 metres mean diameter. The gas inside this ring forms the secondary of a large pulse transformer and carries unidirectional current pulses, of maximum value 200 kA, which last for several milliseconds. This pulse length is much longer than those used by previous workers: the importance of long containment times was emphasized in a previous paragraph. An 'axial' magnetic field is provided by coils wound on the torus and can be varied up to 400 gauss.

It was found that neutrons were emitted from ZETA, and 'ion temperatures' up to 5×10^6 °K were measured spectroscopically. The method of determining the temperature can be explained as follows. The frequency of a wave, as measured by a stationary observer, depends on the relative velocities of the source and the observer; this is the well-known Doppler effect. The same effect also applies in the case of the emission of light by a moving atom: the spectral line is shifted in frequency by an amount which depends on the velocity of the emitting atom. Some of the atoms in a hot gas will be moving away from the observer and some towards the observer. The net result is that the line is broadened, as illustrated in figure 8. If other broadening processes can be neglected, measurement of the line width provides a measure of the temperature.

Obviously, spectral lines emitted by the positive ions must be used if the ion temperature is required. This method cannot, unfortunately, be used directly in a hydrogenous plasma, since, as the hydrogen atom has only one bound electron, its positive ions have none and cannot emit a line spectrum. For this reason lines emitted by impurity ions (oxygen and nitrogen) have been used to measure the ion temperature in ZETA.

Both the detection of neutrons and the observations of Doppler broadening show that energetic ions exist in the discharge. The latter measurements, however, refer to an average ionic velocity in the 'line of sight', and further work is necessary to determine the extent to which the ionic velocities have been 'thermalized'. Also the spectroscopic measurements were made with impurity ions, and their energy may differ from that of the deuterons.

An important point to be remembered is that neutrons can be produced by other mechanisms than a thermonuclear reaction. American workers [19, 20] have found neutrons with a non-isotropic distribution, showing that some kind of accelerating mechanism is operative, a mechanism that may be bound up with the growth of the sausage type of instability shown in figure 9 [20]. For these reasons the Harwell team was cautious and did not claim that a thermonuclear reaction had definitely been produced.

Assuming that the temperature measurements are valid, L. Spitzer [31] holds the view that electron-ion collisions are inadequate to explain the observed rate of heating, and he suggests that co-operative phenomena may be important. It is clear that, while the recent results obtained by P. C. Thonemann's team and by others [32–34] are most encouraging, more experimental work is necessary before it can be definitely stated that a

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thermonuclear reaction has been obtained in the laboratory.

CONCLUSIONS

The study of controlled thermonuclear reactions is in an active and therefore somewhat fluid state. It is not known at present which is the best way to confine a plasma and to heat it to astronomical temperatures. Emphasis has been laid on the pinch effect in this paper, but some other approach may be used. The basic problems which have been discussed with reference to the pinch effect, however, also apply in some measure to other systems.

It is not possible to estimate (but only to guess) the time which will elapse before the arrival of thermonuclear power. What is certain is that the physical properties of a plasma will be elucidated by research in this field and that technical applications will follow in due course. From the scientific point of view, experimental work on plasma is of great interest, since much of the universe exists in this 'fourth state of matter'.

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The scientific work of Emanuel Swedenborg

HERBERT DINGLE

Emanuel Swedenborg is nowadays known almost solely as a religious writer and a mystic, although he was also a diligent student of science with a fertile imagination and was responsible for many original interpretations. A number of aspects of his scientific work, and the reasons for its neglect by both contemporary and more recent scientists, are studied here.

'Everything of importance', wrote A. N. Whitehead, 'has been said before by somebody who did not discover it.' He was referring to those whose minds are too small to comprehend their acts. There are, however, those whose acts are too large for the minds of their contemporaries, and they also may say things of importance which cannot be said thus to have been discovered in the customary sense of the word.

FIGURE 1 – *Emanuel Swedenborg.*

Emanuel Swedenborg was born in Stockholm on 29th January 1688, but the greater part of his early life was spent in Uppsala, where his father, a Lutheran bishop, had made his home. His performances during his schooldays appear to have been creditable but otherwise undistinguished. At the age of 22 he began an extended period of foreign travel, spending nearly two years in London and Oxford and another three years in various European cities. In England he was chiefly interested in the study of mathematics and astronomy. 'I study Newton daily', he wrote, 'and am very anxious to see and hear him.' There is no record that he did so, but he made the acquaintance of Flamsteed, Halley, and various other members of the then young Royal Society. Among other pieces of original work he devised a method of determining longitude from observations of the Moon.

From Paris we have an interesting letter written in 1713, which records that 'Between the mathematicians here and the English there is great emulation and jealousy. Halley, of Oxford, told me that he was the first who examined the variation of the pendulum under the equator; they keep silence about this here; the astronomers here also maintain that Cassini's paper was written before Halley made his expedition to the Island of St Helena, and so forth.'

During these years his inventive faculty was very active, and a list of projects, including proposals for submarines and flying machines, which he drew up suggests a mind like that of Leonardo. There is no record, however, of any construction.

Swedenborg returned to his native land in 1715, full of enthusiasm for the promotion of scientific research in what was then a relatively backward country in this respect. Though greatly interested in mathematical studies, his inclinations appear to have been predominantly practical, and in commenting on the apathy of Swedish mathematicians concerning a proposal to establish an astronomical observatory at Uppsala he wrote: 'The mathematicians . . . have lost all energy and desire to follow up so clever a design, as the one you pointed out to them of the building of an astronomical observatory. It is a fatality with mathematicians that they remain mostly in theory. I have thought that it would be a profitable thing if to ten mathematicians there was added one thoroughly practical man, by whom the others could be led to market; in which case this one

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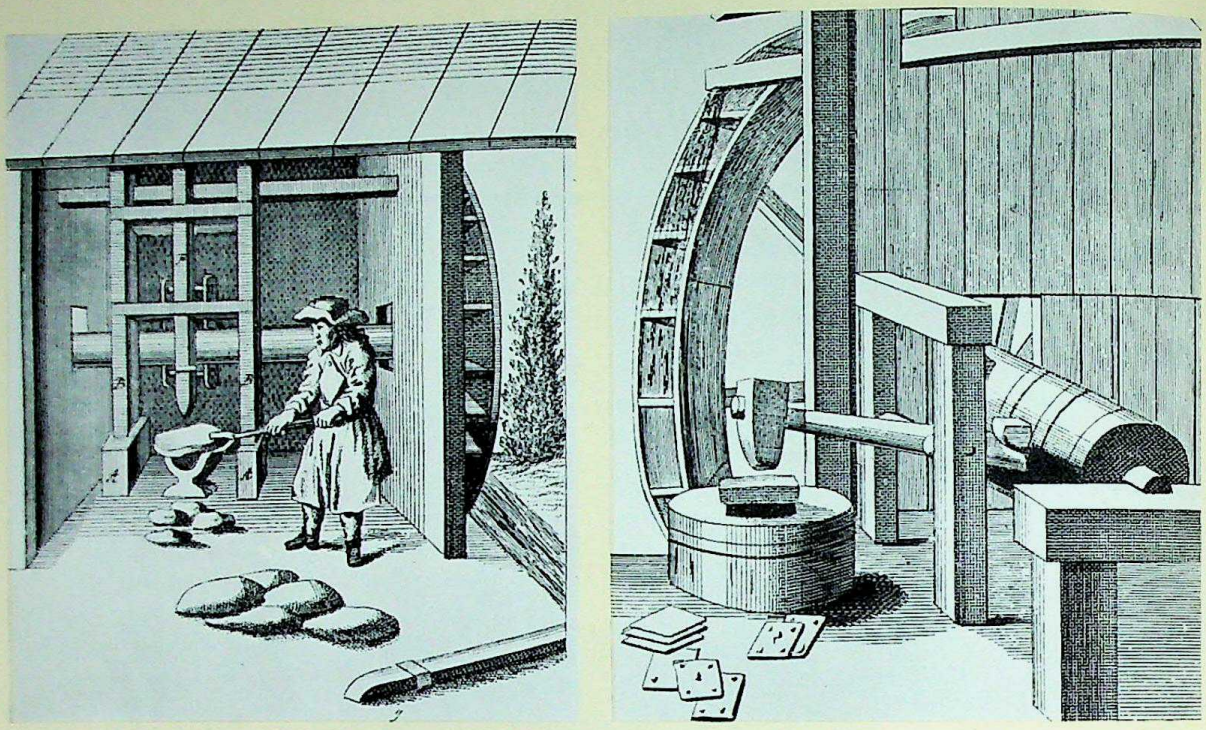
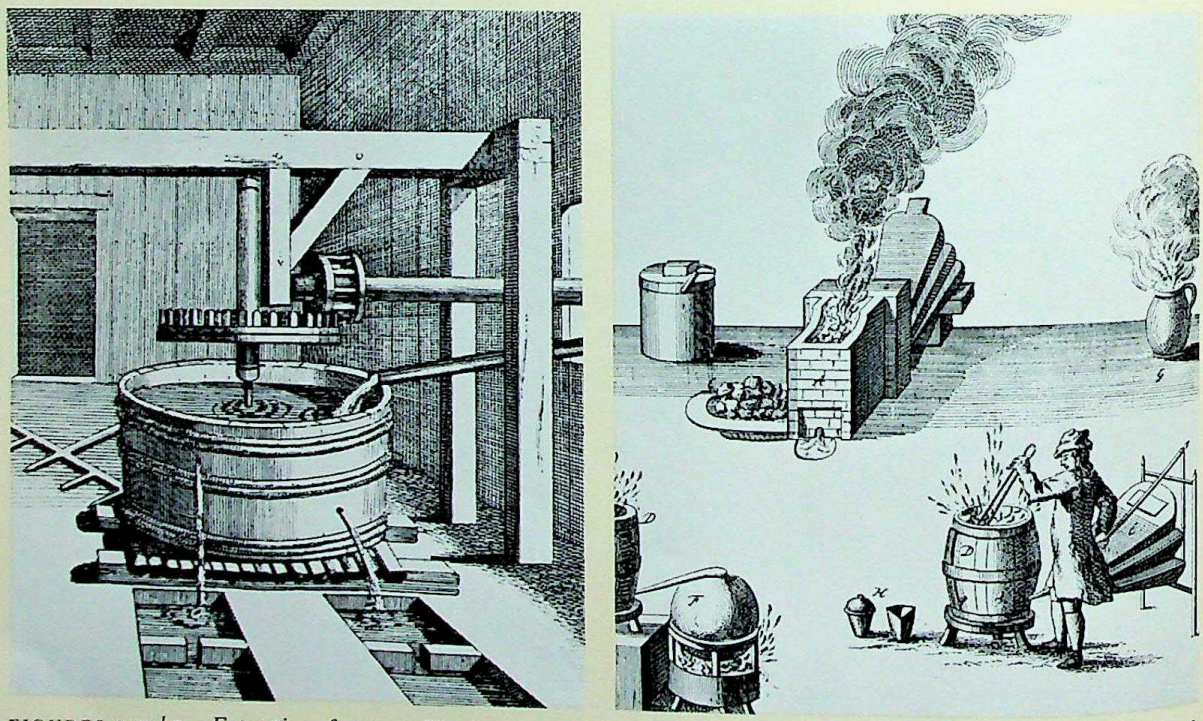


FIGURES 2 *and* 3 – *Water-driven trip hammers. These, and figures* 4 *and* 5, *are from Swedenborg's* Regnum subterraneum, 1734, *a finely illustrated work.*



FIGURES 4 *and* 5 – *Extraction of copper. The first shows enrichment of copper ore by flotation, and the second, apparatus used for assaying copper ores. At the top of this figure a furnace is shown, while the lower diagrams show (left) steam being blown through the roasted ore and (right) separation by flotation.*

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man would gain more renown and be of more use than all the ten together.'

While awaiting a permanent post he started a scientific and technical journal, entitled 'Daedalus Hyperboreus', which, though it received the approval of the King, the famous Charles XII—and, still more remarkable, was read and discussed by him—ceased with the sixth number owing to lack of funds. The young enthusiast, however, was appointed by the King late in 1716 to the post of 'Extraordinary Assessor' at the Board of Mines, which turned out to be so much to his liking that two years later he refused an offer of a professorship of astronomy in the University of Uppsala on the grounds that he could be of more use to his country where he was and that by his tastes and turn of mind he was 'led to mechanics, and will be in future to chemistry', in which subjects 'our Board is noted for having assessors who know very little'.

The death in November 1718 of Charles XII, who had shown great interest in his schemes, was a sad blow, for in Sweden generally they were regarded with almost complete indifference. In the summer of 1721 he started a year's tour of Holland and the German States in order to study the mines and manufactures of those countries. Notwithstanding his meagre success in persuading his countrymen of the importance of such studies as he was pursuing, he was appointed in 1724 as an ordinary assessor to the Board of Mines—that is, to a normal rather than a supernumerary position—and in the immediately succeeding years he published a number of scientific works which won him a European reputation. These were predominantly technical in character, but about 1734 his early interest in the more theoretical aspects of science seems to have reawakened, and from then onwards he gave much attention not only to such fundamental problems in the physical sciences as the ultimate structure of matter, the basic laws of mechanics, the origin of the solar system, and the nature of magnetism, but also to equally fundamental questions in anatomy, physiology, and what would now be termed psychology.

It would be a fascinating study for those with the necessary qualifications, if any such exist, to seek an interpretation in psychological terms of the progress of Swedenborg's dominant interest from the most practical aspects of mining, through chemistry, physics, the mathematical foundations of mechanics, the fundamental principles of physiology, and the nature of the soul, to the ultimate mystical experiences for which he is now chiefly known. Here we can only record the facts.

In the field of practical science his contributions were both topical and profound. The improvements that he introduced into mining and metallurgy of Sweden bear witness to his ability, enterprise, and patriotism, but it is in other directions that his essential originality is better manifested. According to his great fellow-countryman, Berzelius, he was the first—in a paper published in 1719—to point out that the Swedish coast was rising, and the evidence he gives for this involves principles now familiar enough but then new. Sylvanus Thompson ascribes to him the invention of the first mercurial air-pump. Dumas, the French chemist, credits him with originating the conception, basic in the modern science of crystallography, of constructing crystalline forms by grouping spheres, and although there is evidence that this possibility occurred earlier to Kepler, it seems unlikely that the German mathematician associated it with the minute structure of matter as did Swedenborg. It is often impossible to say with certainty who was the first to conceive a particular idea, but these testimonies (and others could be added) from men eminent and well informed in their own subjects make it clear that the fertility of Swedenborg's imagination, of which there is first-hand evidence in his theoretical work, was also operative in the realm of practical invention.

Without losing its interest in this, however, his mind automatically sought for the principles that determined what was and what was not possible in nature. His *Principia*, which forms the first part of his *Opera Philosophica et Mineralia*, published at the expense of his friend the Duke of Brunswick-Lüneburg in 1734, is a treatise as fundamental in character as Newton's *Principia*. Both writers propose principles of motion based on primary philosophical concepts, but whereas Newton changed the medieval postulate (partly accepted by Galileo) that perfect natural motion was circular to the postulate that it was rectilinear, Swedenborg preferred the spiral form—which may be regarded as a combination of a circular with an outward linear motion. On this basis he constructed a theory of the structure and movements of matter, leading to a view of the origin of the solar system identical in many respects with that later propounded by Laplace and generally known as the nebular hypothesis. His nomenclature, which has not come into general use, naturally gives his writing a flavour now chiefly familiar to us in the productions of woolly-minded cranks, but

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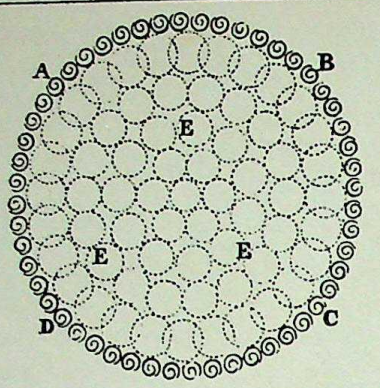

FIGURE 6 – *Swedenborg's conception of 'the first elementary particle' ('atom' in our terminology), in which the circles EE represent 'actives' and the surrounding spirals 'finites'. These have remarkable resemblances to our nucleons and electrons.*

when the inevitable initial repulsion has been overcome it is seen to express clear and penetrating thought. The course of mechanical evolution is worked out in great detail, and the principles are applied to the explanation of magnetism. Here Swedenborg clearly anticipates the modern molecular theory of magnetism, and gives diagrams which could be reproduced with propriety in present-day textbooks on the subject. He brings his theoretical views into close relation with experiments, particularly with those then being made by Musschenbroek, and shows how they explain the phenomena that were progressively being discovered.

The conceptions explored in the *Principia*, though fundamental to physical science, were not yet fundamental enough for Swedenborg, to whom the distinction between physical science and knowledge as a whole was of little significance. His attention was next directed to the working of the human organism, first the body, and then the soul. Well aware that success was to be achieved not by speculation but by prolonged study of the facts and reflection upon them, he sought and obtained, in the years following the publication of the *Principia*, extended leave of absence, and travelled in various countries, absorbing the knowledge of anatomy and physiology available there. Though here, as in the physical sciences, he strongly insisted on experiment as providing the basis of knowledge, and indeed performed a few experiments himself, he relied in this work chiefly on the observations of others. 'Here and there', he wrote when publishing the results of his deliberations, 'I have taken the liberty of throwing in the results of my own experience; but this only sparingly, for on deeply considering the matter, I deemed it best to make use of the facts supplied by others. Indeed there are some that seem born for experimental observation, and endowed with a sharper insight than others, as if they possessed naturally a finer acumen; such are Eustachius, Ruysch, Leeuwenhoek, Lancisi, etc. There are others again who enjoy a natural faculty for contemplating facts already discovered, and eliciting their causes. Both are peculiar gifts and are seldom united in the same person. Besides I found when intently occupied in exploring the secrets of the human body, that as soon as I discovered anything that had not been observed before, I began (seduced probably by self-love) to grow blind to the most acute lucubrations and researches of others and to originate the whole series of inductive arguments from my particular discovery alone; and consequently to be incapacitated to view and comprehend, as accurately as the subject required, the idea of universals in individuals, and of individuals under universals. Nay, when I essayed to form principles from these discoveries, I thought I could detect in various other phenomena much to confirm their truth, although in reality they were fairly susceptible to no construction of the kind. I therefore laid aside my instruments and restraining my desire for making observations, determined rather to rely on the researches of others than to trust to my own.'

The biological results were published in two works, *Œconomia regni animalis* (1740) and *Regnum animale* (1744–45). Here *anima* bears the general meaning of the soul, and only in a secondary sense refers to what we call the animal kingdom. In the introduction to the latter work he wrote: 'I intend to examine, physically and philosophically, the whole anatomy of the body. . . . The end I propose to myself in the work is a knowledge of the soul; since this knowledge will constitute the crown of my studies. . . . In order therefore to follow up the investigation, and to solve the difficulty, I have chosen to approach by the analytic way; and I think I am the first who has taken this course professedly. To accomplish this grand end I enter the arena, designing to consider and examine thoroughly the whole world or microcosm which the soul inhabits; for I think it is in vain to seek her anywhere but in her own kingdom.'

His belief that he was the first to approach the subject of psychology in the manner of 'the experimental philosophy' is probably justified, but lest this should be interpreted, in these days of minute specialization, as evidence of mental aberration, it must be pointed out that Swedenborg's quest

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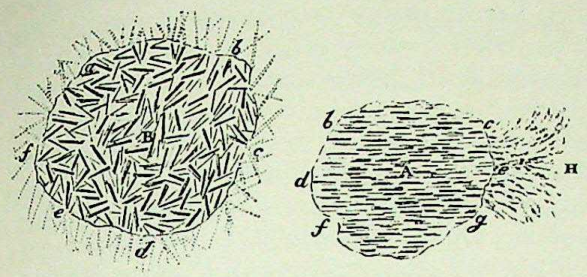

FIGURE 7 – *Swedenborg's diagrams of the 'vorticles' (our 'molecules') in (left) unmagnetized and (right) magnetized iron. This and figure* 6 *are from a modern edition of Swedenborg's* Principia.

was quite in keeping with the scientific spirit of the age. Readers of Newton's *Principia* will recall that he writes: 'This most beautiful system of the sun, planets, and comets, could only proceed from the counsel and dominion of an intelligent and powerful Being'. Further on he says, referring to this Being, 'to discourse of whom from the appearances of things, does certainly belong to Natural Philosophy.' When Swedenborg set before himself the task of studying the soul through the human organism he was proposing a more modest inquiry than that adumbrated by Newton, and—though he was later to discourse of God also—a more narrowly scientific one. There is nothing at all here to indicate that Swedenborg was later to become anything but a typical scientist.

I am not qualified to appraise, or even to describe, Swedenborg's biological work. In his Presidential Address to the International Congress of Anatomists held at Heidelberg in 1903, Gustaf Retzius declared him to be the discoverer of the localization of the functions of the brain. Quoting from Max Neuburger, he stated that 'he leaped a whole century ahead of his age by the announcement of another discovery, for he was the first one to show that the cortical substance of the brain is the exclusive seat of the higher psychical activity.' He has also been credited with the discoveries that the body respires through the lungs, and of the osmotic permeability of living membranes.

The *Regnum animale* was never completed. Swedenborg's plans were changed by the reception of experiences of a mystical character which, though they did not immediately lead to the abandonment of his scientific work, ultimately turned his attention wholly to the contemplation of things outside the scope of the intellect. In 1743, when applying for further leave of absence to pursue his anatomical studies, he wrote:

'I am influenced interiorly by the desire and longing to produce during my life-time something real, which may be of use in the general scientific world and also to posterity, and in this way to be useful to and even to please my native country; and, if my wishes are realized, to obtain honour for it. . . . It is my own chief desire to bring this work to a close, and to return to my country, to my office, and to my property, where I shall, in tranquillity and ease, continue my larger work, the *Regnum minerale*, and thus be of actual use to the public at large in those matters which properly belong to the Royal Board.'

At this time, therefore, it is clear that he was contemplating an eventual return to his mining interests. But in the Preface to his *Arcana Cœlestia*, published in 1749, we read:

'It has been granted me, now for several years, to be constantly and uninterruptedly in company with spirits and angels, hearing them converse with each other, and conversing with them.'

The change he later described as a gradual one, beginning in 1743, when he was 55 years old. At any rate, almost simultaneously with the completed portions of *Regnum animale* in 1745 he published a book entitled *De cultu et amore Dei*, which, notwithstanding its title, belongs more properly to his scientific than to his religious productions. It deals with the Creation of the World, as described in the book of Genesis, which the author, like Newton and other religious scientists of those days, accepted as an account of actual historical happenings. In the *Arcana Cœlestia*, however, the same events are no longer regarded as facts but as allegories. From that time on, Swedenborg's interest in the rational interpretation of phenomena may be said to have disappeared. Natural events became significant only as symbols of eternal spiritual verities, and the remainder of his writings, which have secured him a far greater measure of recognition than his scientific achievements, were concerned with nature only in that aspect. Here, for our present purpose, we take leave of him. All that need be added is that in 1747 he resigned his position with the Royal Board of Mines, and thereafter lived mostly abroad, finally settling in London, where he died on 29th March 1772.

Considered on the purely intellectual level, there is no doubt that Swedenborg was a very remarkable man. His versatility manifests the breadth, as his original discoveries show the profundity, of his concern with natural phenomena. As would be expected, he did not allow the intensity of his search for knowledge to destroy his sense of the

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importance of understanding what that search signified, as, alas, so many do now, He had a clear idea of the philosophy of science, and it is one that later developments have continued to justify.

'There are two ways', he wrote in 1740, 'by which to trace out those things in nature which lie either open before us, or are hidden from our eyes—viz. the *a priori*, which is also called the synthetical method, and the *a posteriori*, or the analytical method. Both are necessary in reflecting upon and tracing out one and the same thing: for in order to do so there is required both light *a priori*, and experience *a posteriori*. Now, while the learned among the ancients followed the former light as remotely and profoundly as they possibly could, those at a later period were induced not to accept anything as witness, unless it was confirmed by experience. Hence also some of the learned at the present day seem to have agreed to let thought rest, and to make experiments which would appeal to the senses; yet they did so with the hope and intent that some day experience would be connected with theory: for experience deprived of an insight into the nature of things is knowledge without learning, and a foundation without a building to rest upon it. The observations of the outward senses merely furnish data and give information about things which the understanding ought to investigate, and concerning which it ought to form its judgments.'

Why is it that such a gifted and successful investigator has left so small a mark on scientific history? The number of his discoveries that have had to be repeated later before being recognized as such is probably greater than that of any other man, and the lack of recognition was not because they were in any way inaccessible. How has such a remarkable thing happened?

I think it is because his eyes were fixed on a point too far ahead. He leaped to the goal by intuition, but was unable to explain the way in such terms that his contemporaries could tread it. To contribute to scientific progress you must see ahead of your fellows, but not too far ahead. If someone could have performed the Michelson-Morley experiment in the sixteenth century, Copernicus would have been discredited. If Newton had realized that his experiments did not testify that acceleration was absolute, we might have had no inverse square law of gravitation. If Darwin could have been convinced that acquired characters could not be inherited, we might not have had his theory of evolution by natural selection. The advance of science depends on the solution of each problem as it presents itself, regardless of what is to follow, and the discernment of a bee-line to the goal is useless to those who have feet but no wings. The piecemeal discovery of a devious but pursuable course is the way of progress. Swedenborg, like Browning's Grammarian, aimed too high for temporal success: his discoveries had all to be made again later, by other methods.

Science succeeded where medieval philosophy failed because it restricted its scope. Ceasing to ponder on God, on nature, and on human life, it timed the rolling of balls down slopes and scanned the rise of a liquid in a glass tube. By such a self-denying ordinance it discovered how trivialities worked, and left weightier matters to later generations, justified by the conviction that knowledge of the trivialities was necessary for the future task. Science may thus be said to have arisen from the realization of the impossibility of philosophy.

Swedenborg never attained to that realization. He accepted the science but could not acknowledge the impossibility.

'In respect to the soul and its various faculties,' he wrote early in the *Principia*, 'I do not think it possible that they can be explained or comprehended by any of the laws of motion known to us; such indeed is our present state of ignorance, that we know not whether the motions by which the soul operates on the organs of the body are such as to be reducible to any rule or law, either similar or dissimilar to those of our mechanics.'

Newton might have written that, but he did not allow the inapplicability of his laws to the organs of the body to prevent him from announcing them as universal laws of non-living matter. Swedenborg could not be satisfied with laws that did not cover the soul as well, and his aim was to derive completely universal laws. He failed, as any human being must have failed; but that, after all, is only half the story. He had his reward, though it was not a place of pre-eminence in the temple of scientific history.

NOTE. For various reasons it has not been possible for me in one or two cases to verify from the original sources the statements ascribed in this article to other writers. My information is taken from 'Swedenborg: Life and Teaching', by G. Trobridge (Swedenborg Society; fourth edition, 1935), which, so far as I have been able to check it, has been found completely trustworthy.

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Science and technology in the Brussels Exhibition

The Brussels Exhibition, now in its third month, is the first of its kind to be held in Europe since before the war. One of the principal tasks of its organizers has been to make an inventory of the material wealth of humanity, and it is therefore not surprising, in view of the tremendous developments of the last twenty years, that science and technology receive more attention than on any previous occasion of a similar kind. The principal national pavilions show much scientific material, although they naturally vary considerably in the emphasis laid upon it, and in addition the International Hall of Science illustrates—irrespective of the country of origin—many of the great scientific discoveries of recent years. Before the Exhibition closes it will have been seen by some forty or fifty million visitors, and therefore it is, quite apart from other considerations, a major contribution to the very important task of interpreting science to the general public and of illustrating its relationship to everyday life.

On such occasions each participating nation seeks not only to display its achievements but to convey something of its way of life and modes of thought. While the highly artificial atmosphere and limitations of space of any exhibition inevitably prevent it being truly representative, the observant visitor to Brussels has a unique opportunity of comparing one nation with another and learning something of the complex world in which we live.

Such a conspectus can be of considerable practical importance, quite apart from being of great general interest. In this connection the Great Exhibition held in London in 1851, the ultimate ancestor of the present world fair in Brussels, deserves our consideration, for it is sufficiently a part of history for its significance to be clearly discerned. For Britain it was an event of great consequence, for it determined an important part of national policy for many years ahead. In 1851 the international jury awarded Britain prizes in almost all the hundred categories of manufacture. To the complacent, this was sufficient evidence of easy supremacy, but the more perceptive of British observers saw in the exhibition alarming evidence of strong competition from abroad. This observation had the important result of initiating in Britain a concerted effort for more and better technical education, the great importance of which had already been realized by most other European nations. The Paris Exhibition of 1867 showed how well justified had been the forebodings of the critics: then, only sixteen years later, Britain gained barely a dozen awards. This result greatly assisted those who were working for educational reform, and although the lack of success was disappointing the effect of the exhibition was salutary, and in the long run was much to the advantage of British industry. It is fair to say that its effects are still felt.

This is an old story and relates to a world very different from that now represented at Brussels. It is, however, worth recalling as a reminder that international exhibitions are not ephemeral affairs for the entertainment of the masses but can, if properly interpreted, usefully influence national policies for many years ahead. If the Brussels Exhibition has such an effect it will serve a most useful purpose. Some lessons are already clear. One, most forcefully conveyed, is that relatively backward nations who wholeheartedly foster the development of science and technology can in a remarkably short space of time challenge, and in some respects surpass, those who have been much longer in the field but have failed fully to appreciate the importance of this new branch of learning. Conversely, it shows that those which have neglected science and technology do so to their detriment, for they remain backward. It strikingly illustrates, too, that if science is properly used it can bring great benefits to mankind and is by no means the destructive force it is still too often supposed to be. The evidence of the Exhibition must greatly strengthen the arguments of those who, in many countries, are urging the need for the inclusion of more science in general education.

At the same time it must not be forgotten that the Brussels Exhibition is designed to display man's spiritual as well as his material wealth, and it is a useful reminder that technical progress alone is sterile. It is particularly pleasing to see how often exhibits of a technical nature have been satisfactorily united with others of a more general character, for it is of the first importance to show that science is a part of our modern culture and no longer stands in isolation.

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Ocean waves

G. E. R. DEACON

The great practical and theoretical difficulty of studying ocean waves has resulted in their being much less intensively studied than most other forms of wave motion. During the past decade, however, important progress has been made, and this is here reviewed. It seems clear that further advance depends in the main upon more theoretical and experimental research on the basic physical processes. Such research promises results of interest not merely to scientists but to sailors, ship designers, and civil engineers concerned with coastal works.

INTRODUCTION

The great practical and theoretical importance of the regular oscillations that constitute wave motion needs no emphasis. The study of such diverse subjects as acoustics, optics, radio telegraphy, atomic structure, tides, and diseases of the heart, has demanded close investigation of wave forms, and it is surprising that the waves that have been by far the longest known, and give the name to the whole genus, have been studied in detail only comparatively recently.

The delay has, however, not been without good reason. Electromagnetic and acoustic waves are more easily studied than sea waves. Realistic patterns of wind and waves are not easily reproduced in the laboratory, and the difficulties of making observations at sea are formidable. In the absence of precise information about the nature of the phenomenon, theoretical workers had to be satisfied with the solution of rather formal problems. One textbook aptly describes the early theoretical work as mathematically beautiful rather than practically applicable. While a closer relationship between theory and observation would have been fruitful, progress had to wait for recent advances in hydrodynamics and electronics, particularly in the study of turbulent flow patterns and the theory of random signals.

Progress depended also upon a practical stimulus powerful enough to provide the considerable funds and the specialists required, which stimulus was given by the military needs of the last war. It was then necessary, especially for the great landings on the north coast of France, to improve methods of predicting the general pattern of waves from meteorological charts and forecasts and to learn something about the changes in offshore submarine bars. Subsequently interest has extended to problems of coastal engineering and the study of ships' motion. As the subject has become more manageable more scientists have been attracted, with the result that today steady progress is being made. The purpose of this article is to review what has already been achieved and to indicate the lines of research most likely to be fruitful in the future.

The greatest progress has been made within recent years, but it would be unjust to earlier workers, especially those of the nineteenth century, to ignore their contributions. A well-known feature of sea waves is that they are not of uniform amplitude. Traditionally, every seventh wave is an exceptionally high one, although Ovid gave this distinction to the tenth. In fact there is no such simple rule, but there is sufficient regularity in the alternation of high and low groups of waves as they approach a beach to suggest close comparison with the well-known beating of two sound waves. Beating presupposes a mixture of waves of different wavelengths: sometimes they get into step and reinforce each other, sometimes they are out of step and partly neutralize each other. To apply this conception to sea waves, however, involves theoretical and experimental difficulties that could not be faced until very recently. Although as long ago as 1876 G. G. Stokes compared high and low groups of sea waves with sound beats, his classical theoretical work and that of F. Gerstner, A. L. Cauchy, S. D. Poisson, Kelvin, Rayleigh, and others was based on the assumption that there was a single train of waves with a simple harmonic profile. In 1947 H. U. Sverdrup and W. H. Munk [1] acknowledged the interference of waves, but they and most others attempting wave prediction had still to assume that there is a dominant wavelength of larger or 'significant' waves which is recognizable and for which theoretical and empirical relationships can be developed.

Great waves are very impressive, and measurements of their height are of general interest. During six years of recording at a point a mile from the north coast of Cornwall the highest wave

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was eleven metres, but fifteen metres is not uncommon in the open sea. Eighteen-metre waves have been recorded on one occasion (figure 3).

WAVE ANALYSIS

According to modern views, the sea surface can be considered as the sum of an infinite number of simple wave trains of different frequencies, with a continuous frequency spectrum. Local wind conditions and the travel of waves from distant storm areas determine how at any particular point the energy is distributed among the different wavelengths. Changes in the spectrum and of the energy contents of different wave bands can now be accurately estimated, the degree of accuracy depending on the length of the record and the rate at which the wave pattern is changing.

The first serious attempt to make a spectrum analysis of sea waves seems to have been made in 1944–45 by an oceanographical team at the Admiralty Research Laboratory, Teddington. They used records of wave height obtained from three recorders laid on the sea bed along a line running out to sea from the north coast of Cornwall; they were respectively 1000, 2000, and 3000 yards from the shore. Even though the recorders were comparatively so close together, it was found that the interference between waves of different wavelengths and different speeds was so great that waves and groups of waves identified on the record of one instrument could not be recognized on that from the next. This result confirmed observations of waves travelling in deep water where an individual wave crest cannot be followed very far with the eye before it loses its identity.

These Cornish experiments led to the conclusion that there could be little improvement in the prediction of wave patterns until the distribution of energy between the various wave-bands could be computed with the same precision as in other branches of physics.

The first sea-wave analyser was described by N. F. Barber, F. Ursell, J. Darbyshire, and M. J. Tucker [2] in 1946. A wave record in the form of a black profile on photographic paper 14 cm wide was stretched round the outside of a drum 76 cm in diameter. A strip of light fell transversely on the record, and the intensity of the scattered light was measured with a photocell. In this way an electrical reproduction of the wave profile was obtained when the drum was rotated. The electrical signal was fed into a fixed-frequency filter, covering a narrow band, the output of which was rectified and recorded on another paper strip. The drum was set spinning and its speed allowed slowly to decrease under the effect of friction. In this way each wavelength in the record came in turn into resonance with the filter and the recorder drew an amplitude spectrum. To calibrate in terms of wave period, a second record, containing known wave periods, was simultaneously analysed by another optical system and filter on the opposite side of the drum.

The important innovation in this instrument was the continuous decrease in the speed of rotation of the drum. With this apparatus a complete Fourier analysis took about fifteen minutes, care having to be taken to make the rate of change of frequency match the characteristics of the filter, so that full resonance was obtained for each harmonic. Attempts to speed up the process led to difficulties. In modern forms of the instrument (figure 2) the speed of the drum is controlled by reference to the exponential discharge of a capacitor through a high resistance. It has also been made considerably smaller so that it can be taken to sea; a brief account has been written [3].

Many alternative techniques have been advocated which can usefully supplement our simple procedure, but this has proved far too useful to be lightly given up. It gives the information in the form in which it is needed for the prediction of swell and gives more than can be obtained by simple autocorrelation. It has led to many advances, such as the formulation of a new theory of microseisms and an extension of the theory of ship movements from the treatment of model ships in regular waves to that of real ships in complex wave patterns.

Obtaining wave records for analysis is no longer a serious problem. Tucker [4] has described apparatus that has been used to measure waves at the coast, on the sea bottom a mile offshore, from aeroplanes, and from ships. A wave recording buoy is shown in figure 4. Measurement from ships is the most striking development of all. It allows measurements while moving [5] by an ingenious combination of pressure recorders and accelerometers. These measure the varying pressures at two points below water on opposite sides of the ship; coupled accelerometers make allowance for the vertical motion of the measuring points.

Such wave recorders have already been fitted in ten ships—three British, three French, and four American—and already a vast amount of material for analysis has been collected. D. E. Cartwright [6] has used these shipborne recorders to deduce the direction of travel of waves from the apparent

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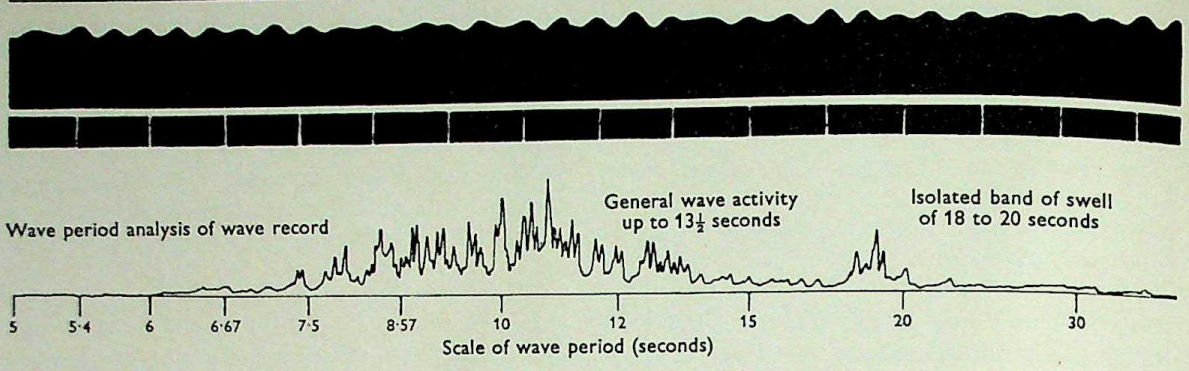

FIGURE 1 – *A wave record (top), with its wave period analysis immediately below.*

changes in frequency as the ship steams round a regular twelve-sided figure. Combining the results with simultaneous recordings of roll, pitch, and heave, he and L. J. Rydill [7] have made an important extension to the theory of ships' motion.

WAVE SPECTRA

Analysis of waves has proved very informative. Figure 1 shows part of a typical wave record and the corresponding wave spectrum. Most of the recorded wave energy is contained in waves with periods between seven and fourteen seconds; the remainder is in a separate band of longer waves with periods between eighteen and twenty seconds. The interpretation of such spectra in terms of prevailing meteorological conditions is often difficult, but important general conclusions can be drawn from the analyses of waves produced during comparatively simple meteorological conditions. Such waves behave as though they originate in a large number of point disturbances distributed throughout the area over which the wind blows. The maximum wavelength corresponds to the greatest wind strength; as will be seen, this correlation between wavelength and the strength of the causative wind is very important.

What happens at a time of active wave generation is not clear, but there must be some sort of interaction between various wave components resulting in energy being transferred from short to long waves as the dominant wavelength increases. After the storm is over, however, the interaction seems to cease and all the component wave trains seem to travel independently of one another. The result is a wave pattern which at any instant is the sum of a large number of wave trains having random phase relationships and covering more or less the same range of wavelengths. These wave trains spread out about 30° to 40° on either side of the predominant wind direction.

In figure 1 the spectrum begins with waves having a period of six seconds. This is because the movement in a wave decreases rapidly with depth, being reduced to about one-twentieth of the surface value at a depth of half a wavelength. This particular spectrum was made with a pressure recorder lying on the bed of the sea at a depth of about 25 metres, and the components of shorter wavelengths were not recorded. The general activity ends with waves having a period of fourteen seconds, because the causative wind, measured 30 feet above the surface, was about 28 knots. The waves with periods of eighteen to twenty seconds could be attributed to a distant storm in which the wind exceeded 40 knots.

Such a storm produces wave trains comprising a full range of waves with a periodicity up to twenty seconds. Those with periods less than eighteen seconds have not reached the recording station at the time of figure 1 because of their relatively slow speed of travel. As the disturbance spreads out from the storm centre the longer waves get farther and farther ahead of the shorter. The first swell to arrive on a distant shore has a period of about twenty seconds, and even after allowing for the increase in height due to shoaling it may have an amplitude of only a few centimetres and a wavelength of several hundred metres. It may not

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be visible except over shallow ground, where it becomes slower and the crests become closer and steeper; it would then be called ground swell. Later parts of the record will reveal the slower, shorter swell. The highest swell will have a period of about fifteen seconds. After the arrival of this, the swell will gradually decrease in both amplitude and wavelength until the effect of the distant storm dies away in the shortest waves that can travel so far without being completely dissipated.

More precisely, the component wave trains, if not too steep, travel independently from the storm area with the group velocities appropriate to their periods. If the generating area is well defined and far enough from the recorder the successive wavelengths can be distinguished. The distance of the storm centre can be calculated from the difference in arrival time of longer and shorter waves. The implicit assumption that the component wave trains travel independently, with the velocities predicted by classical wave theory, is justified by the fact that the computation proves correct whenever it is made in meteorological circumstances which clearly provide only one storm centre.

Disturbances attributable to specific storms may travel immense distances. When there are no storms in the North Atlantic, wave recorders installed off the Cornish coast can be operated at their maximum sensitivity. The wave spectra then often show a band of swell the periodicity of which diminishes so slowly that the calculations based on relative velocities and difference in time of arrival leave no doubt that it is generated in storms near Cape Horn. It is usually only a few centimetres in height. Similarly, records made recently on the coast of California have demonstrated the arrival of swell generated south of New Zealand.

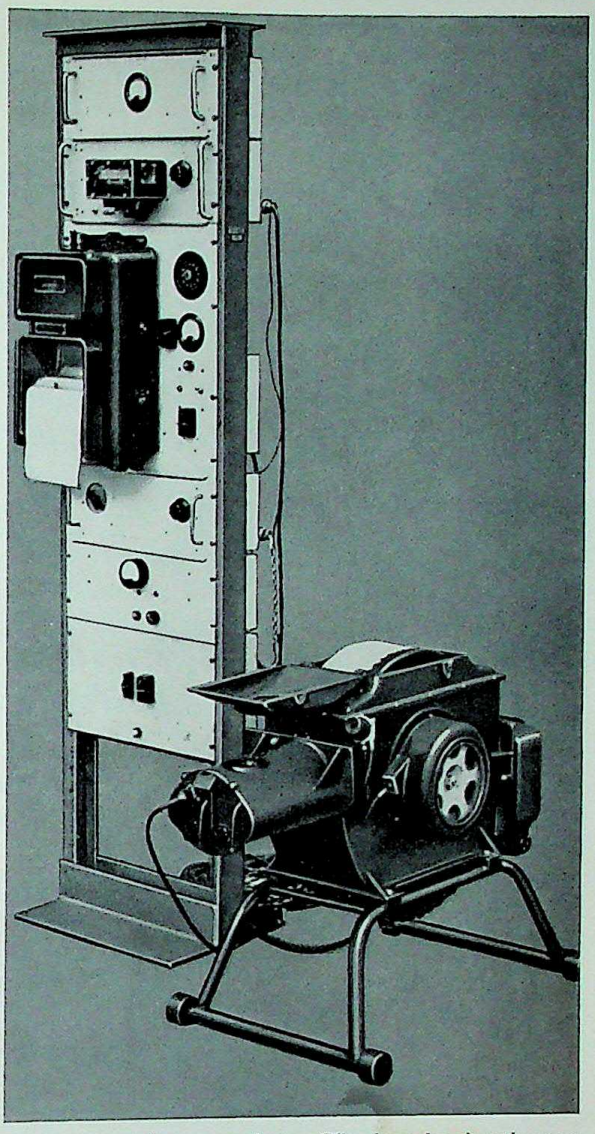

FIGURE 2 – *A wave analyser. The drum bearing the wave record can be seen on the right.*

METHODS OF WAVE PREDICTION

As has been shown, wave spectra can in appropriate circumstances be related to meteorological conditions. Conversely, it ought to be possible to predict the wave pattern at any particular point from the relevant meteorological data; waves derive their energy from the winds, so that if we have wind charts and forecasts we should be able to predict the waves. In fact, however, progress along these lines has not been quite up to expectation. The wave predictions made for the landings on the Normandy beaches during the last war were remarkably accurate, but greater difficulty has been experienced in forecasting for truly ocean coasts and for islands in mid-ocean. Here the newly acquired information about the rates of travel of different wave-bands has been useful. Although all the storm energy may be communicated to the waves in half a day, its arrival at a distant coast may be spread over several days. The spectral distribution also affects the rate of decay of the wave energy because shorter components lose energy more quickly.

A serious limitation to progress is our lack of knowledge of the physical processes by which the energy of the wind is communicated to water. It is believed that the main responsible factors are the development of positive pressures on the windward slopes of waves and some reduction of pressure on the leeward slopes, and the frictional drag of the wind over the waves; but we have little or

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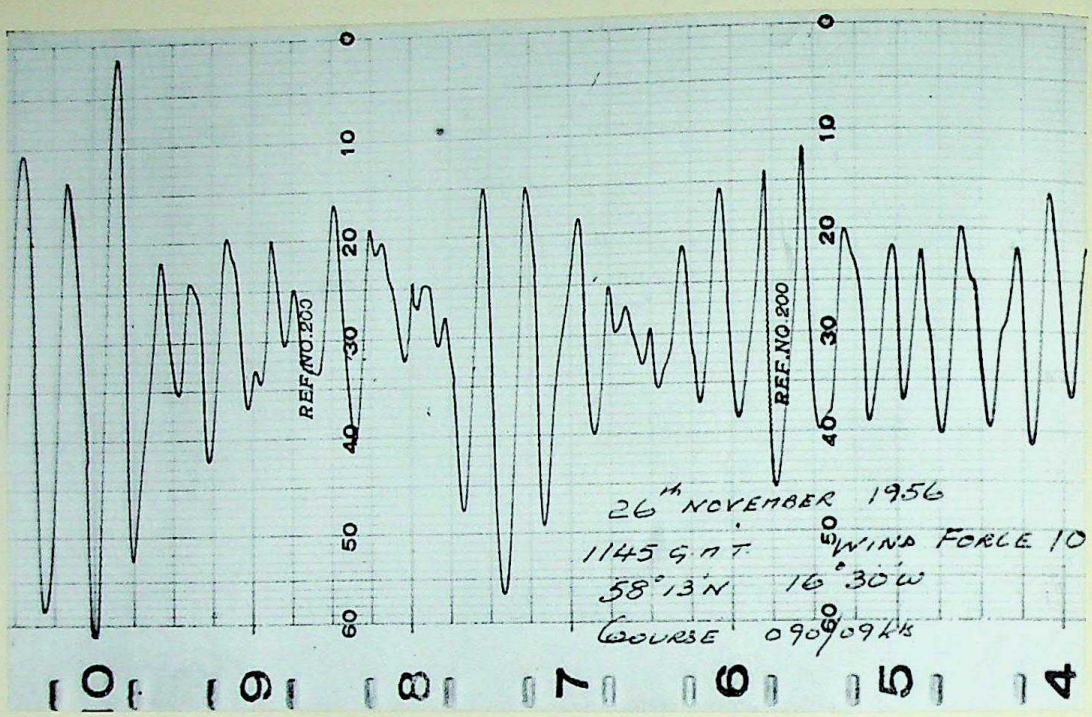

FIGURE 3 – *A record of some large waves, taken by the Ocean Weathership 'Weather Explorer', on 26th November 1956. The vertical scale on the record measures wave height in feet, and the high wave on the left is about 60 ft from crest to trough. The horizontal scale has four divisions to 1 minute.*

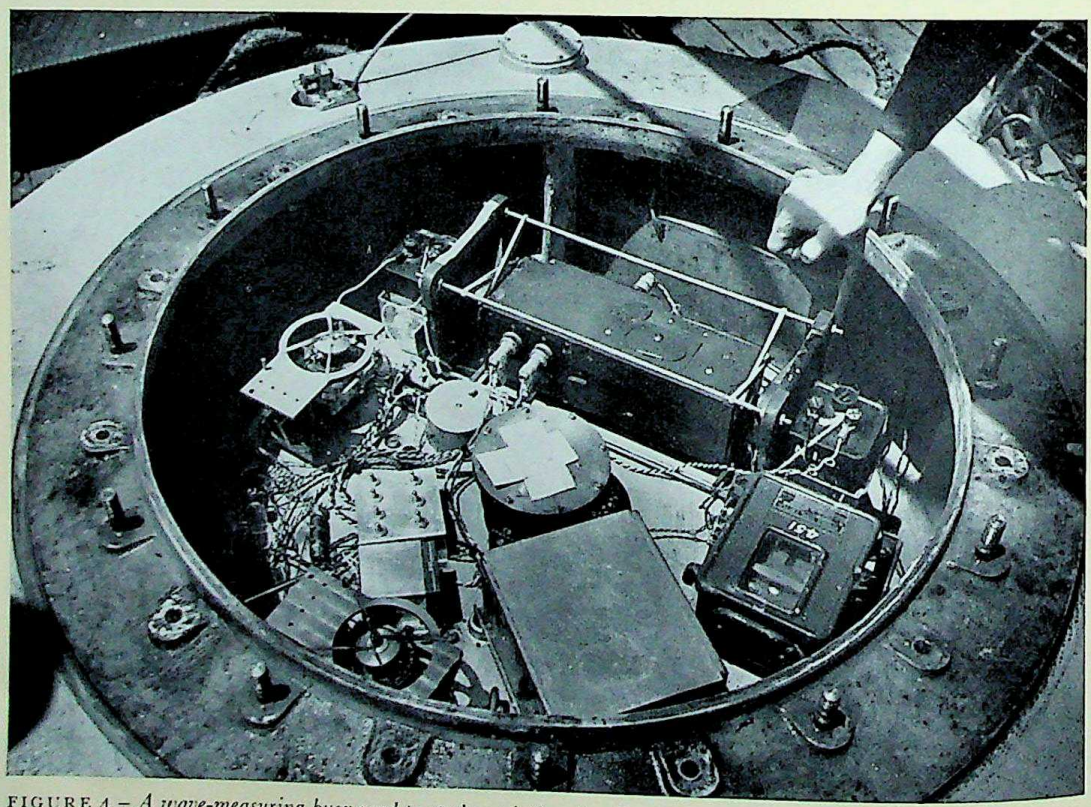

FIGURE 4 – *A wave-measuring buoy used to study variations in air pressure close to the sea surface in relation to the heights and slopes of the waves. The two gyroscopes on the left provide records of the slopes of the waves in the direction of the wind and at right angles to it; the round box in the centre contains a vertical accelerometer, and behind it is the recording camera. In the foreground is the electronic apparatus for doubly integrating the accelerometer output to give the wave profile and for recording the output of the microbarograph which is contained in the lid, which has been removed. Most of the batteries are hidden under the rim. Records were obtained in winds of up to 35 knots.*

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no real information, though the necessary measurements are now being made. The gradual diminution in the height of waves as they leave the storm area may be due partly to air resistance, but the spread of waves as they move outward and the dissipation of energy by viscosity or turbulent decay seem to be more important.

The problem has been approached empirically through the analysis of meteorological data and wave records, but the results are disappointing, mainly because the observations are made on such a wide range of natural phenomena and are affected by variable depths, tides, and other factors. Real progress will probably be made by combined theoretical and empirical approaches. Work by O. M. Phillips [8], for example, gives a reasonable picture of the early stages of wave generation in terms of known physical quantities, e.g. the normal pressure fluctuations due to atmospheric turbulence.

SOME RECENT ADVANCES

The fact that we can now make reliable frequency analyses has stimulated studies of waves themselves, of ships' movement, and of microseismic ground oscillations. M. S. Longuet-Higgins [9] and J. Darbyshire [10] have shown that microseismic ground oscillations are caused by the interference of sea waves travelling in opposite directions behind rapidly moving storms, or by the interference of advancing waves and those reflected back from a coast. It seems likely that measurements of microseisms can be used to obtain useful information about distant storms and approaching swell even in the absence of meteorological charts for the region in question.

A picture of the sea surface as a two-dimensional random process has been suggested by W. J. Pierson in America and by M. S. Longuet-Higgins [11] in Britain, and they have worked out many of the statistical distributions connected with such surfaces. New techniques have been developed for measuring such properties as the intervals between crests in any direction, the number of points of reflected sunlight per unit area and the statistical distribution of their velocities, and the lengths and directions of wave contours. These will give us two-dimensional spectra and reveal much about the distribution of energy in the surface. C. Cox and W. H. Munk [12], for example, have made a statistical survey of the distribution of energy in the sea surface from photographs of sun glitter. Better knowledge of the nature of the surface should assist research on the mechanisms of the processes by which wind energy is converted into wave energy.

With growing knowledge have come increasingly important practical applications. Statistical work has contributed, for example, to the study of ships' motion. Cartwright and Rydill have been able to calculate, from the wave spectrum, the probabilities of the occurrence of maximum amplitudes, velocities, and accelerations of movements in ships' structures.

There have also been notable achievements in the study of beach currents due to waves. This has included water transport due to waves [13], movements of sand on the sea bottom [14], and the rip currents so dangerous to bathers [15].

In the latter connection F. P. Shepard found evidence of strong outward currents at various points on all coasts where there are large breakers. They usually consist of narrow streams that run out through the breakers and fan out beyond them. They can occur in places where there is little tidal movement and can be recognized from the air or from high cliffs as an outward movement of discoloured water or foam. If caught in a rip current, the swimmer can escape by swimming parallel to the shore.

ACKNOWLEDGMENT

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The origin of the solar system

SIR HAROLD SPENCER JONES

Astronomers have long been interested in theories of the origin of the solar system, and these theories can be divided into two groups. Theories of both groups, the one assuming that the system has been formed by the slow evolution from a primordial mass, and the other based on the idea of a catastrophic interaction of the Sun and another celestial body, are examined here, and the probable development of a satisfactory theory is suggested.

It has long been recognized that the solar system shows so many conspicuous regularities that it could not have been formed by chance. The planets revolve around the Sun and rotate around their axes in the same sense, which is that of the Sun's rotation. Their orbits, which are of small eccentricity, are nearly coplanar. Most of the satellites also move in orbits of small eccentricity that are inclined at small angles to the equator of the associated planet, and their motions are in the same sense as those of the planets. The outer satellites of Jupiter and Saturn and the satellites of Neptune are, however, exceptions. The distances of the planets from the Sun show some degree of regularity, represented approximately (except for Pluto, the most distant planet) by the empirical law known as Bode's law.

The planets can be divided into two groups: the four inner planets are of relatively small mass (the Earth being the most massive) and of high mean density (4·2–5·5 g/cm^3); the four major planets are much more massive and are of low mean density (0·7–1·6 g/cm^3). Pluto, the outermost planet, is of small mass, and it seems probable that it was originally a satellite of Neptune.

A remarkable feature of the solar system is that almost all its angular momentum is in the orbital motions of the major planets (98 per cent), while almost all its mass (99·87 per cent) is concentrated in the Sun.

During the past two centuries many theories have been proposed to account for the origin of the system. These theories can be divided into two groups. One group supposes that the system was formed by the slow evolution of a primordial mass; the other group is based on the assumption that it was formed as the result of interaction of a catastrophic nature between the Sun and another celestial body. The early hypotheses about the origin of the system illustrate these alternative views. Buffon in 1745 suggested that the planets were formed from fragments of matter torn off the Sun as a consequence of its having been struck by a huge comet. Kant in 1755 supposed that the Sun was originally at the centre of a nebula, which was in rotation round the Sun under its gravitational attraction. Mutual collisions between the separate particles caused the nebula to flatten out into a disk, within which a gradual condensation around the denser aggregations ensued. These aggregations, each in rotation, condensed further to give rise either to a planet or to a planet with a system of satellites. Laplace in 1796, starting from the same hypothesis as Kant, supposed that the nebula was originally hot and that as it cooled it gradually contracted, its rotation becoming more rapid because of the conservation of its angular momentum. When the centrifugal force at the equator became greater than the gravitational pull, a ring of matter split off round the equator. This process was repeated several times, each of the rings aggregating into a single mass; in some of these a similar process was supposed to have occurred, so giving rise to the satellite systems.

There are many objections to the hypotheses of Kant and Laplace. Among them is their failure to account for the distribution of angular momentum in the system. If, for instance, the whole of the angular momentum of the system were concentrated in the Sun, its period of rotation would be about 12 hours; but the centrifugal force at its equator would then be only about 5 per cent of the force of gravity, so that rings of matter could not possibly be thrown off as supposed by Laplace.

In order to meet the angular momentum difficulty, many attempts have been made since the beginning of this century to develop a satisfactory theory based on the action of an external body. Such action would involve changes in the distribution of the angular momentum, so that it

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is conceivable that a mechanism by which the planets could be formed and endowed with their large angular momentum might thereby be provided. The earliest of these attempts was the planetesimal hypothesis developed by T. C. Chamberlin [1] and F. R. Moulton [2], who supposed that at some remote epoch another star passed very close to the Sun, causing a succession of huge eruptions to occur on it, each accompanied by the ejection of matter. The ejected matter was pulled round by the attraction of the passing star, more or less in the plane of the relative motion of the two bodies. As the matter cooled, it condensed into liquid drops which quickly solidified, forming the planetesimals. The denser portions of the matter are supposed to have formed the nuclei of the planets, which grew by accretion of the planetesimals as they circled the Sun. The residual matter formed a resisting medium, the effect of which was gradually to reduce the eccentricities of the orbits.

H. Jeffreys [3] pointed out that the frequency of collisions between the planetesimals, and their high relative velocities, would cause them to be volatilized long before accretion could have any important effect. He and J. H. Jeans [4] modified the theory by supposing that the passage of the star near the Sun drew out a long curved filament of matter, which was unstable and broke up into a number of separate fragments in orbital motion round the Sun. Some of these fragments, while still only partially condensed, were broken up on their first return to the vicinity of the Sun, so producing the satellites. Some of the matter that was drawn out fell back on the Sun, giving it a rotation in the same direction as the orbital motions. The theory in this form did not stand detailed examination. It could not, for instance, meet the angular momentum difficulty.

The hypothesis of a stellar encounter was modified in various ways in order to overcome these objections, thereby becoming increasingly improbable. Jeffreys [5] supposed that instead of a near approach of the star to the Sun there had been an actual collision. H. N. Russell [6], however, showed that on the most favourable assumption the angular momentum per unit mass of the matter from which the planets are supposed to have been formed would have been less than one-tenth of the average angular momentum per unit mass of the actual planetary system. To overcome this difficulty he suggested that the Sun might originally have been a binary star and that the intruding star collided, not with the Sun itself, but with its smaller companion. R. A. Lyttleton [7] investigated this problem and found that, under certain circumstances, both the colliding star and the Sun's companion could escape from the system leaving sufficient material to form the planets, while at the same time the angular momentum difficulty could be overcome.

The material drawn out from a star by a near approach or by an actual collision with another star would be at a very high temperature, so that the mean velocity of the atoms would be very large. L. Spitzer [8] showed that under these conditions the filament would expand under its own internal pressure much more rapidly than it would lose energy either by radiation or by turbulent convection. The filament would not condense into separate planets, but some of it would escape into interstellar space while the remainder would form an extended gaseous envelope around one or more of the stars involved. None of the modifications of the hypothesis of the close encounter or collision of the Sun and another star can meet this objection. All further attempts to account for the origin of the solar system have been based on the hypothesis that it has been produced by the gradual evolution of a primordial system. Such theories all start from the assumption that the Sun was at the centre of a diffuse cloud of gas and dust, which may have been an interstellar cloud that the Sun had entered or a cloud from which the Sun itself had condensed or was in the process of condensing.

A theory of the origin of the solar system has been proposed by H. Alfvén [9] based on the assumption that the Sun formerly possessed a large magnetic field and was in the last phase of its formation, with about its present temperature. The gaseous cloud surrounding the Sun was ionized by its radiation. The degree of ionization of any particular element would depend on the temperature and therefore on its distance from the Sun. In an inner region surrounding the Sun all the elements would be ionized, but, going outwards from the Sun, the progressively decreasing temperature would result in helium, hydrogen, oxygen, carbon, silicon, iron, and magnesium, among the principal constituents of the cloud, ceasing successively to be ionized.

The magnetic field around the Sun would tend, according to Alfvén, to impede the fall of the ionized constituents towards the Sun under its gravitational attraction, so that the chemical composition of the cloud would depend on its distance. Any given constituent, in its fall towards the Sun,

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would reach a distance at which, because of its increasing temperature, ionization would set in. Any further fall would be stopped at a distance roughly proportional to the distance at which gravitational energy is equal to the ionization energy.

When any constituent became ionized, the electromagnetic forces would tend to accelerate it to the same angular velocity as the Sun. A magneto-hydrodynamic transfer of angular momentum from the Sun to the cloud would take place. The ionized gas would move along the magnetic lines of force and eventually be brought into the equatorial plane, where it served as the raw material for the formation of planets. Within a certain region around the Sun, gravitation would prevail, so that the material in that region would fall into the Sun. Alfvén gave reasons for supposing that four main clouds would be produced by this process, differing in composition and consisting respectively primarily of helium, hydrogen, carbon, and iron with silicon, though there would be no clear-cut separation between these clouds.

The theories of the processes by which planets and satellites are supposed to have been formed while the Sun was growing progressively by accretion were developed in great detail by Alfvén but need not be described here. The difficulty of this theory is that the magnetic field of the Sun, which was postulated as the agent that caused the material surrounding it to be suspended against gravity for long periods of time while the diffusive separation of different elements was taking place, is required to exceed a certain critical value. This critical value is 300 000 gauss, whereas the present magnetic field of the Sun is only about one gauss even at the surface. In addition, no adequate discussion was given of the mechanisms by which the magnetic field could suspend the cloud against gravity and by which the gravitational energy, as the cloud fell in, could be converted into thermal energy and energy of ionization.

C. F. von Weizsäcker [10] formulated a theory which was largely qualitative but which emphasized the importance of hydrodynamical considerations and of the effects of turbulence. He assumed that the Sun was at the centre of a local gas cloud, extending to about the present limits of the solar system and having a mass about one-tenth that of the Sun, which had condensed from an eddy in the general galactic gas cloud. Collisions between the various eddies in this cloud caused them to acquire angular momentum. The Reynolds number[1] for such a system is very high, of the order of 10^{11}, so that turbulence developed which, together with internal friction, changed the shape and orientations of the paths of the particles until they were eventually reduced to orbits of nearly circular shape in the vicinity of the equatorial plane of the Sun. Viscous forces came into play which, by slowing down the faster-moving inner parts and speeding up the slower-moving outer parts of the cloud, tended to equalize the angular velocities of the different parts of the cloud. There was thus a gradual transfer of angular momentum outwards, the material with low angular momentum becoming concentrated towards the centre, so that the slowly rotating central mass was surrounded by a faster-rotating flattened gaseous cloud.

The turbulent pattern was superposed on the Keplerian rotation. Particles of equal mean motion would collect into vortices and Weizsäcker assumed that the vortices would arrange themselves into a succession of rings, the most stable arrangement being obtained with an integral number of vortices in each ring. Between successive rings there would be steep velocity gradients, giving rise to large viscous stresses. Secondary eddies, in the nature of roller bearings, would form on the circles separating the main vortices, the rotation in these eddies being in the opposite direction to that in the vortices. Weizsäcker showed that condensation would start in these roller bearings and that in the later stages growth would occur by gravitational attraction when the mass exceeded a certain critical value. As there would never be perfect symmetry, the condensations would be of different sizes. The larger condensations would sooner or later attract the smaller, and eventually the eddies in each ring would come together and form a proto-planet. It was suggested that an analogous sequence of events would follow in the atmospheres of these planets, giving rise eventually to the satellite systems.

By assuming empirically that there were five vortices in each ring Weizsäcker was able to obtain a satisfactory representation of the relative distances of Mars, the asteroids, Jupiter, Saturn, and Uranus. S. Chandrasekhar and D. ter Haar [11], however, have shown that the

[1] This number, involving the velocity, density, linear dimensions, and viscosity of the fluid in motion, gives the conditions under which motions are similar when the qualities in question vary. In many cases flow which is laminar for low values of Reynolds number becomes turbulent for higher values.

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eddies postulated by Weizsäcker could not arise from a turbulent medium.

The basic ideas of this theory have been used by G. P. Kuiper [12] to develop a modified theory, in which an important role is attributed to the process of gravitational instability. He supposes that the Kolmogoroff spectrum of turbulence[1] can be applied and concludes that large eddies may be found anywhere in the cloud and that small eddies may occur inside as well as outside the larger eddies. The eddies have comparatively short lives and are continually forming and dissolving. In order that condensations may take place it is essential that their density should exceed the critical value at which the internal attractions within the cloud are equal to the solar tidal force, for otherwise disruption by tidal action would occur. Kuiper supposes that the formation of planetary masses occurred essentially at the critical density for each of the condensations.

These condensations formed proto-planets, which at first were essentially of cosmic composition and which extended to about half the width of the ring from which they had drawn their material. Their masses must have been greatly in excess of the present masses of the planets, though their distances were approximately those of the present planetary distances. Kuiper considers that Bode's law merely represents the initial radial density distribution in the solar nebula.

In order to explain how the proto-planets subsequently lost most of their mass, with only the present planets surviving, Kuiper supposes that they were formed by contraction, before solar radiation was important, during the Helmholtz-Kelvin period, i.e. before the period of thermo-nuclear heating. Comparison between the roughly known energy-content of the proto-planets and of the present planets indicated that much energy had to be added to the proto-planets to make possible the evaporation of most of their material, and this energy could only have been provided by the Sun. He considers that after the Sun had completed its contraction and a rapid rise in its temperature ensued, the surrounding cloud became ionized and the angular velocity of the Sun was reduced by magnetic coupling to the ionized material in contact with it. Solar corpuscular radiation swept out the ions; the vacated spaces were replenished hydrodynamically from the denser regions and the new material was in turn ionized and swept away. Thus the entire interplanetary space was gradually cleared. Hot exospheres then formed in the outer fringes of the proto-planets, followed by evaporations to interplanetary space, the material so evaporated being continually swept clear. The evaporation of the proto-planet envelopes can plausibly account for the present planetary obliquities, except for that of Uranus.

The rotations of the proto-planets would initially synchronize with their orbital revolutions. As the proto-planet contracted, the rotation could no longer be synchronized and then became direct.

The five largest satellites (the four Galilean satellites of Jupiter and Titan of Saturn) were formed within the proto-planets by a process similar to that by which the planets were formed. The other satellites were of hybrid origin, starting with the collection of solid condensed matter before gravitational effects could become important. The subsequent loss of mass of the proto-planets led to a proportional expansion of the satellite systems. Some of the outer satellites could then have been lost to interplanetary space and subsequently either have been recaptured by the proto-planets to become irregular satellites or have become Trojan asteroids.

The outermost region of the solar nebula had a density below the limit at which proto-planets could be formed. Condensation products, such as water, ammonia, methane, etc., in the solid or frozen state must have formed in that region, because of the low temperature, and then have slowly collected to form larger aggregates. Kuiper suggests that these aggregates formed the comets, which were subsequently scattered through the system by the perturbing action of the planets.

The Soviet scientist, O. Y. Schmidt, has published a series of papers [13] on the origin of binary stars and of the solar system. He supposes that the Sun in its revolution round the centre of the galaxy passed through a cloud of interstellar matter and captured a great amount of it. Such capture is possible only if the velocity of the Sun relative to the cloud is small. In general this would not be so, and Schmidt infers that relatively few stars could capture clouds and develop planetary systems.

The cloud captured by the Sun consisted partly of gas and partly of dust particles and, because of its low density, must have had dimensions many times greater than the present solar system. The cloud contracted gradually under the gravitational attraction of the Sun. The particles of the

[1] According to Kolmogoroff, the eddy sizes in turbulent medium are distributed according to the law

$$F(\lambda)d\lambda = C\lambda^{-1}d\lambda$$

where λ denotes the eddy size.

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cloud would have had the same general direction of motion around the Sun, but their individual orbits varied greatly in their size, in their elongation, and in their angle of inclination to the central plane of the cloud. Collision between the particles would eventually bring them into a flattened disk, consisting of dust and solid bodies, resembling Saturn's rings, revolving round the Sun and surrounded by a cloud of gas.

The larger particles increased in size through collection of smaller particles, and numerous aggregations capable of resisting the Sun's disruptive tidal force began to form inside the disk. The collisions resulted in decrease in the relative velocities of the particles and a consequent shrinkage of the aggregations, while a considerable part of the kinetic energy was transformed into heat and radiated into space. Those particles which, through collision, acquired very elongated orbits were captured by the Sun at their near approach, so that the Sun gradually acquired a slow rotation in the direction of the revolution of the cloud. The aggregations were considered by Schmidt to be the 'embryos' of the planets. If two embryos were moving along comparatively close orbits, the one nearer the Sun would grow primarily by acquiring particles with smaller specific angular momenta, while the one farther from the Sun would grow primarily by acquiring particles with larger specific angular momenta, so that their orbits would gradually shift towards and away from the Sun respectively. Schmidt showed that if a sufficiently large fraction of the original kinetic energy of the particles were transformed into heat, the planetary aggregation would acquire a direct rotation.

Schmidt gave reasons for concluding that, given a smooth distribution of matter in the initial cloud, the specific angular momenta of the planets should increase by equal steps outwards from the Sun. The specific angular momentum of a body revolving around a central mass is proportional to the square root of its orbital radius. Hence the square roots of the orbital radii of the planets should increase approximately in arithmetical progression. Such a relationship holds approximately for the inner planets, Mercury to Mars, and also for the outer planets, Jupiter to Pluto, the step from one planet to the next being much greater for the outer than for the inner planets. This is attributed to the difference in the properties of the inner and outer zones of the cloud. It is doubtful, however, whether these relationships have any real physical significance. Pluto, in particular, must for various reasons, be considered anomalous.

Schmidt's arguments are largely qualitative and it seems much more probable that the initial stage of planetary formation consisted of large protoplanets, which lost much of their initial material, as required by Kuiper's theory, rather than small embryos which grew by attraction and accretion of matter, as supposed by Schmidt.

The problem of formulating a satisfactory theory of the solar system cannot be regarded as yet solved, though important advances have been made in recent years. The theories that involved the close approach of another star to the Sun, or an actual collision have been abandoned and it seems reasonably certain that an explanation has to be sought in the evolution of an extended system containing gas and solid particles. Among the nearest stars about one half are known to be binary or multiple systems and the formation of a planetary system may be a special case of the formation of such systems. A fundamental difficulty is that the problem is not susceptible of exact mathematical analysis. The theory developed by Kuiper, though not entirely free from objection, appears at present to provide the most satisfactory explanation of the main features of the solar system and may with some modifications prove capable of leading to a theory that can be generally accepted.

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Parasite behaviour and the control of insect pests

GEORGE SALT

The main weapons at present in use in the attack on insect pests are the chemical insecticides. Biological control, which employs the natural enemies, predators or parasites, of the pest, is also of considerable importance and may have advantages in certain circumstances. It necessitates an extensive knowledge of the physiology and behaviour of these enemies: this article discusses relevant studies of the behaviour of some parasitic wasps.

The development and use of DDT, benzene hexachloride, and similar insecticides since 1945 has attracted so much attention to the chemical control of insect pests that other methods of control have been greatly neglected. There are signs, however, of reviving interest in other approaches to the problem. An increasing number of insect pests are developing resistance to insecticides. The cost of spraying rises as more concentrated poisons have to be applied. Some pests, for instance those widespread over great tracts of wild country and those that live inside plant tissues, are not easily accessible to chemical attack. For these and other reasons there is a place in the armoury of the economic entomologist for every means of combating pests, among them the method of biological control.

Natural enemies destroy large numbers of insects; the object of biological control is to enlist them to attack the particular insects that we call pests. If these allies are to play their full part in the control of insect pests they must be used with understanding. The efficient modern insecticides were developed from a vast programme of chemical research, and are used with detailed knowledge of their chemical properties. It is naive to suppose that natural enemies can be effectively used for control with inadequate knowledge of their biological nature, especially their physiology and behaviour. The purpose of this article is to consider some of the problems of behaviour that are met in using certain parasites, the ichneumons and related groups of parasitic wasps, for the control of insect pests.

When an ichneumon is ready to lay eggs it flies to a suitable habitat and there searches for a host. If we watch a female *Apanteles glomeratus*, a typical ichneumon, seeking larvae of the cabbage butterfly, *Pieris brassicae*, on young plants of brussels sprouts in our garden, we shall see it fly from leaf to leaf and walk briskly on their under surfaces. When it finds a young larva, it makes a careful examination and, if satisfied, pushes its ovipositor through the skin of the caterpillar and rapidly lays a number of eggs in the body cavity. Then it withdraws and prepares to attack another host. If all goes well, the eggs will develop, destroy the caterpillar, and provide a new generation of parasites to continue the good work.

Although the many details of behaviour involved in finding, choosing, and parasitizing a host are usually performed correctly, *glomeratus* often makes mistakes both of omission and commission. This parasite appears to seek larvae of *Pieris brassicae* only on cabbages, sprouts, and other cultivated brassicas; it fails to find them when they are feeding on certain other food plants, such as capers, *Tropaeolum*, and *Cakile maritima*, the sea rocket. Therefore, although almost all of the caterpillars on cabbages may be destroyed, the pest remains untouched on these other food plants, and survives to breed an abundant population in the next generation [1]. Sometimes, on the other hand, the parasite lays some of its limited number of eggs in larvae that are useless for the purpose, such species as *Pieris rapae* and *Mamestra brassicae*, which it comes across while seeking its proper host on garden brassicas. Long ago, N. F. Meyer [2] showed that the eggs the parasite mistakenly lays in *P. rapae* are often wasted, for even in caterpillars so closely related to the proper host many of the parasite eggs are unable to develop, and that part of the parasite egg-population is lost.

The behaviour of a parasite while choosing its host can easily be observed in the laboratory by the use of *Trichogramma* (figure 1), a ubiquitous chalcid-wasp, which does immense but inconspicuous good by destroying large numbers of pests while they are still in the egg stage. *Trichogramma*

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is useful all over the world against a wide variety of insect pests: against the sugar-cane moth-borer in the West Indies, the cotton bollworm in Rhodesia, the oriental fruit-moth in Canada, and so on. There can be few parts of the world where it does not occur. For several years I have made a practice of collecting a few egg-masses of the cabbage moth, *Mamestra brassicae*, from the plants in my garden. After a few days indoors some, but by no means all, hatch, for every year from many of them, sometimes more than half, there emerge the tiny adults of *Trichogramma*. These run about in the corked vial, and if a fresh egg-mass of *Mamestra* or other similar moth is put with them, they soon settle down to lay their eggs in the new hosts. The behaviour of this small creature has been studied in detail and turns out to have some aspects which decrease its value as a controlling agent and others which make it more efficient than one might expect.

On the one hand, when choosing hosts in which to lay its eggs, *Trichogramma* makes what seem to us most absurd mistakes [3]. It will drill into empty eggshells from which the larval pest has already hatched. It will examine and try to drill into various small objects of about the same size as its usual hosts—such things as plant seeds, globules of glass, or even grains of sand—when these are pasted on cardboard and put before it like so many moth eggs on leaves. A most amusing experiment to watch was one devised to discover the limits of size of objects *Trichogramma* would accept as hosts. A globule of mercury smaller than a host egg was put into a petri dish with a female parasite, but attracted no attention. Minute droplets of mercury were run into the globule until it reached a size the parasite would accept. The female would then climb upon it, examine and accept it, and attempt to push in her ovipositor. The surface tension of the mercury was too great, however, and the little parasite would sit, sometimes for hours on end, gently bouncing up and down in her attempts to drill into this strange host (figure 2). While this was going on, other droplets of mercury could be added to the globule, which eventually reached such a size that the parasite no longer recognized it as a host, and moved off. T. E. Holloway [4] and P. Marchal [5] have observed *Trichogramma* attacking and actually laying eggs in small droplets of a gummy substance exuded from leaves of *Hibiscus*. This misguided behaviour must detract from the value of the parasite as a controlling agent by wasting part of its short adult life and some of its limited number of eggs.

In another respect the behaviour of *Trichogramma* is unexpectedly efficient. Let us consider a problem of the interaction of parasite and host populations, the sort of problem always involved in projects of biological control. Let us suppose there are 100 individuals of a pest in a unit of space, and that the available parasites attacking them have 100 eggs to distribute in that space. How many of the hosts will be destroyed?

We can calculate that, if the parasites distributed their eggs one at a time at random, on the average about 37 hosts will receive one egg; 18·5 will receive two eggs, 6 three, and 1·5 four. This inefficient distribution of the eggs would allow 37 pests to escape unparasitized. If, on the other hand, the parasites could distribute their 100 eggs with complete efficiency, all of the 100 pests could be destroyed. For this second situation to obtain, the parasites would have to be able to distinguish hosts already parasitized and avoid laying in them, so conserving all their eggs for the remaining healthy pests. What can we learn about the behaviour of *Trichogramma* in this respect?

The first experimental evidence [6] was obtained by a simple arrangement of hosts. A piece of graph paper ruled in tenths of an inch was cut in a circle to fit into the lid of a petri dish. Twenty-five healthy moth eggs were pasted within a quarter of an inch square, and the five rows and columns were numbered to facilitate reference to each egg (figure 3). The eggs used were those of a grain moth, *Sitotroga*, which were chosen because they are just sufficient in size for the development of one parasite. A female *Trichogramma* was put into the inverted petri dish, and a record was kept of every contact between the parasite and a host. It was soon apparent that discrimination between healthy and parasitized hosts was taking place. When the parasite touched a healthy host, it climbed upon it, examined it with its antennae for about 30 seconds, drilled through the eggshell, and about three minutes from the first contact went off to seek another. When, however, the parasite touched a host it had already parasitized, the examination lasted only five or seven seconds, and the parasite went off without further ado.

In each experiment the parasite was watched until it had made 25 attacks. Throughout the first part of an experiment the discrimination of the parasite appeared to be perfect. Towards the end, when most of the hosts had already been attacked and the parasite had run about for a long while without finding one of the few remaining healthy hosts, its urge to lay seemed to break down its

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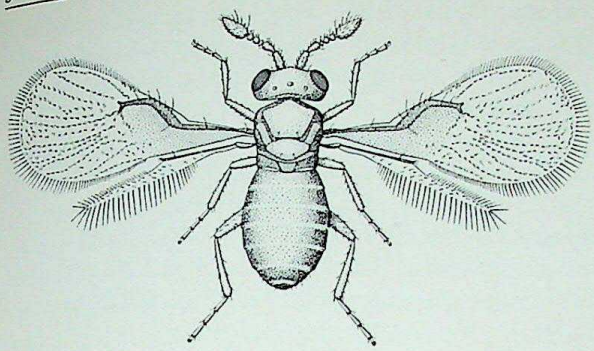

FIGURE 1 – *The egg-parasite* Trichogramma. (× about 40)

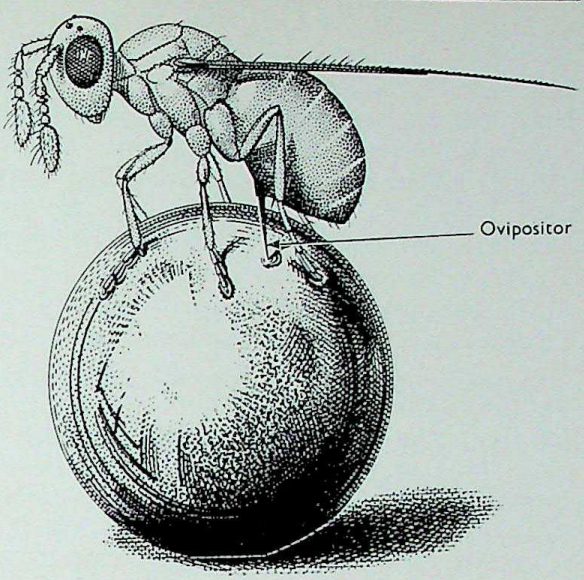

FIGURE 2 – Trichogramma *attempting to drive its ovipositor into a globule of mercury.* (× about 64)

restraint, and occasionally it accepted and drilled into a host already parasitized. Even in these cases, however, the parasite frequently withdrew its ovipositor as soon as it had penetrated the egg-shell and went off without laying a second egg. This could be shown by subsequent examination of the host. In five such experiments, each parasite being watched until it had distributed 25 attacks among 25 hosts, an average of 3 mistakes were made, most of them after twenty hosts had already been parasitized. The conclusion is inescapable that the parasite was able to distinguish between healthy and parasitized hosts, and to restrain itself from laying in the latter. Nevertheless, when most of the hosts were parasitized its restraint eventually broke down, and it occasionally attacked hosts already parasitized.

One immediately wants to know how the parasite was able to distinguish between healthy and parasitized hosts. The fact that hosts already parasitized are rejected after a cursory examination lasting only a few seconds shows that there is some external indication. In order to discover its nature, twelve eggs of *Sitotroga* were pasted on alternate squares of a graph-paper arena, as shown in figure 4*a*. A female *Trichogramma* was allowed to examine each of these hosts in the manner that normally precedes oviposition, but just as it accepted one and was about to unsheathe its ovipositor it was struck aside with a fine brush. These 12 hosts had been examined but not parasitized; let us call them 'dirty' hosts. The parasite was then removed and 13 clean, healthy hosts were pasted in the intervening squares of the graph paper so that a group of 25 hosts was formed as in the previous experiment (figure 4*b*).

A second parasite was then put into the dish and was kept under observation until it had made 25 attacks. In the few cases when the parasite accepted a 'dirty' host, it was pushed aside when it unsheathed its ovipositor, so that the host remained unparasitized. When it accepted a 'clean' host it was allowed to lay its egg, but while it was drilling into the next clean host it met, the one previously parasitized was removed and another clean egg was put in its place. The substitution

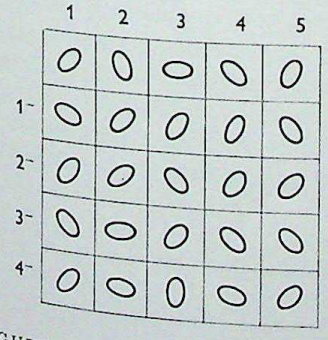

FIGURE 3 – *Arrangement of hosts on* $\frac{1}{16}$ *in. graph paper for the first experiment.*

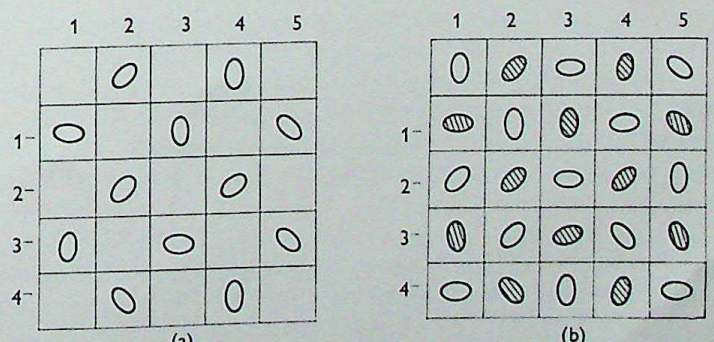

FIGURE 4 – *Arrangement of hosts:* (a) *in first phase of experiment,* (b) *in second phase. 'Dirty' hosts are shaded.*

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was easily made, for the process of oviposition allowed 2½ minutes in which to make the change, and while the parasite was engaged in its attack it was not disturbed by movements of the brush placing a fresh host even in an adjacent square. This method maintained the situation that every time the parasite moved off a host it had just parasitized, it found 12 'dirty' and 12 'clean' hosts about it ready for its next choice.

The behaviour of the parasites used in eight experiments of this sort was very consistent. The 'dirty' hosts were approached and touched but were in nearly every case immediately rejected; the clean hosts were accepted. The conclusion is clear that *Trichogramma* was able to distinguish the hosts that had been merely walked upon by another parasite, and that it avoided attacking them [7].

Further experiments showed something of the nature of the trace that is perceived by the parasite. When 'dirty' hosts were washed with water on a brush and then offered to a parasite, they were accepted as readily as clean hosts. The trace is therefore water-soluble. When a large number of parasites were allowed to walk about in a vial and the glass on which they had walked was washed with a drop of water, the solution so obtained could be used to paint 'clean' healthy hosts, which were then rejected. From all this it appears that, in the act of walking on a host during the examination that precedes attack, the *Trichogramma* female leaves a chemical trace which is perceived by a parasite that comes along later, much as a dog can follow a scent.

Under natural conditions, rain might wash off such an external trace and cause mistakes. It is therefore interesting to find that *Trichogramma* has a safeguard in the form of a second discriminatory sense. When a parasite was allowed actually to parasitize a number of *Sitotroga* eggs and those eggs were then washed and offered to another parasite, they were examined and accepted as if they were healthy. But as soon as the ovipositor had pierced the eggshell and entered the host, the parasite usually pulled it out and ran off at once, without laying a second egg. Sense organs on the ovipositor appeared to warn the parasite that all was not well. Referring back to the first experiment, it was presumably this second sense that inhibited the laying of another egg when the parasites made mistakes and attacked hosts already parasitized.

This account has been restricted to *Trichogramma* because the analysis of behaviour has been carried further with it than with other parasites, but similar powers of discrimination have been demonstrated in several other forms. D. C. Lloyd [8] has shown that two ichneumonids, *Diadromus* and *Angitia*, and a braconid, *Apanteles plutellae*, avoid laying in hosts already attacked by parasites of their own or other species. F. J. Simmonds [9] has studied the similar behaviour of a chalcid, *Spalangia*. Dissection of pests collected in the field has shown that the distribution of several parasites in them is incompatible with a hypothesis of random distribution [6].

The intrusion of this matter of behaviour into our problem of the interaction of parasite and pest populations is thoroughly practical. When the parasites of our numerical example discriminate between healthy and parasitized hosts we can expect to have a more efficient distribution of their progeny than the 63 per cent parasitism that would result from random distribution. But because the restraint of the parasite eventually breaks down we cannot expect to find 100 per cent parasitism. What proportion of the pests will actually be destroyed depends on many factors, but especially on details of the behaviour of the parasites in finding and choosing their hosts.

The conclusion is clear that the behaviour of insect parasites plays a great part in fixing their value as pest controls; and it follows that we must know their behaviour before we can use them efficiently. Therein lies a difficulty, for the necessary investigations have not been done. The proportion of papers on chemical control and on biological control published in the journals of economic entomology since the war suggests that attempts deliberately to use parasites and predators are almost completely out of fashion. Perhaps in the future biologists will be asked to produce parasites to specification for the destruction of particular pests, as chemists develop their synthetic molecules. But a great deal of research, especially on the behaviour of parasites, will have to be done first.

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Ion exchange in clay minerals

C. B. AMPHLETT

Ion exchange is nowadays thought of mainly in terms of organic exchangers. It was, however, first studied in connection with what have since been recognized as ion exchangers in soil more than a century ago, before the concept of the existence of ions in solution had been suggested. Exchanges in clay minerals have recently become of interest in connection with the disposal of radioactive waste. The results obtained are of fundamental significance, and they, and the methods used, are described here.

Ion-exchange properties are exhibited by certain materials which are insoluble in water but are capable of exchanging cations or anions present in their structure for cations or anions dissolved in the external solution; the ion of charge opposite to that being exchanged plays little part in the reaction. There are two well-defined classes of ion exchanger, as well as a variety of miscellaneous materials. One class consists of the organic exchangers, in which the exchangeable ions are bound electrostatically to acidic or basic groups forming part of a large, insoluble organic counter-ion. The other class, the crystalline mineral exchangers, are those in which the exchangeable ions are present in a well-characterized crystal structure which permits exchange between ions of suitable size and charge. In all cases the exchanger as a whole is highly insoluble, but the exchangeable ions are mobile provided that charge neutrality is maintained.

Most present-day studies on ion exchange are devoted to organic exchangers, either the synthetic ion-exchange resins or materials derived by the treatment of natural products such as wood, coal, peat, and others. A flourishing industry has been built around the uses found for these materials, and new applications are constantly being described. Yet long before either these materials or their predecessors, the zeolitic exchangers, were first studied, the foundations of what are now known to be ion-exchange equilibria were investigated in studies of the retention of bases by soils. This plays a vital role in soil fertility and plant cultivation, and has continued to attract attention [1]; in recent years there has been an increased interest in this apsect of ion exchange.

Although it has long been known that the soil possesses purifying properties towards coloured solutions passing through it, the first systematic studies were carried out just over a century ago [2]. During the period 1845–50 experiments were made on the absorption by the soil of ammonia liberated by the passage of water over manure heaps, and the following salient facts were established. Ammonia is absorbed with the release of an equivalent quantity of calcium from the soil; potassium, sodium, and magnesium are similarly absorbed, although to differing extents, and in all cases the uptake is greater at higher concentrations. The absorption power of the soil, which was traced to the clay minerals present, is reduced by prior heating. The property of 'base exchange' is immediately familiar to us nowadays, but it is interesting to note that these typical ion-exchange properties were described and correctly interpreted many years before the concept of ions in solution had been suggested.

Before discussing the subject in more detail, we may note some of the consequences of ion exchange in clays and soils.

1. Cations supplied as fertilizer are retained in the soil against leaching by ground water, and thus remain available to plants over long periods, e.g. when K^+ is applied as kainite ($KCl.MgSO_4.3H_2O$) it displaces calcium from the soil and remains in the plant environment. The naturally occurring cationic plant foods, such as potassium, which are liberated by the weathering of primary rocks (e.g. the feldspars), are retained in the soil by ion exchange on the secondary minerals which result from the weathering.

2. The physical properties of a soil depend to a large extent upon the cations associated with the clay fraction, and may be changed by suitable treatment. Thus, sodium-rich soils are generally unsuitable for agricultural purposes because of their high water capacity, coupled with the excessive swelling tendencies of sodium clays in solution. Calcium-rich soils, on the other hand, give a better crumb-formation and provide a much better tilth. This may be achieved by heavy liming, in which Ca^{2+} ion displaces Na^+ ion to

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give the desired properties. Much of the damage to agricultural soils caused by excessive sea-water flooding is due to the replacement of Ca^{2+} ions by Na^{+} ions, and the process may be reversed by heavy liming or by treatment with gypsum, which, being less soluble than lime, releases its calcium slowly over a longer period.

3. The plastic properties of clays also depend upon the cation present, sodium clays generally being more plastic than the naturally more abundant calcium ones. For brickmaking purposes the properties of clay may usually be improved by mixing soda ash with the clay before firing. In a similar way the civil engineer sometimes improves the qualities of soil foundations by altering the cation composition.

ION EXCHANGE IN CLAYS [3]

One of the principal sources of the property of ion-exchange displayed by soils is the clay fraction, and it is important to understand at least in outline how this property arises, since the ion-exchange capacities of clay minerals and soils may vary over a wide range (Table 1). The clay minerals form a

TABLE 1

Cation exchange capacity of some clay minerals [3]

Material	Capacity, milliequivalents/100 g
Kaolinite	3–15
Illite	10–40
Montmorillonite ..	80–150
Vermiculite ..	100–150[1]

[1] Commercial vermiculite usually has a lower capacity than this because of the presence of hydrobiotite, which does not exchange.

very complex group of structures, many of which shade almost imperceptibly into each other through a variety of intermediate types. Only three types will be considered here, for purposes of simplicity. Basically, the clay minerals are built up from layer-lattice structures stacked parallel to each other in sandwich fashion, normal to the c-axis of the crystal. The different types of structure arise by variations in the pattern of stacking and by variations within the layers themselves.

Figure 1 shows in a very simplified fashion the way in which the structures of these three types of clay mineral are built up from units composed of tetrahedral $Si(OH)_4$ and octahedral $Al(OH)_6$ layers. In kaolinite, whose structural formula approximates to $Si_4Al_4O_{10}(OH)_8$, there is very

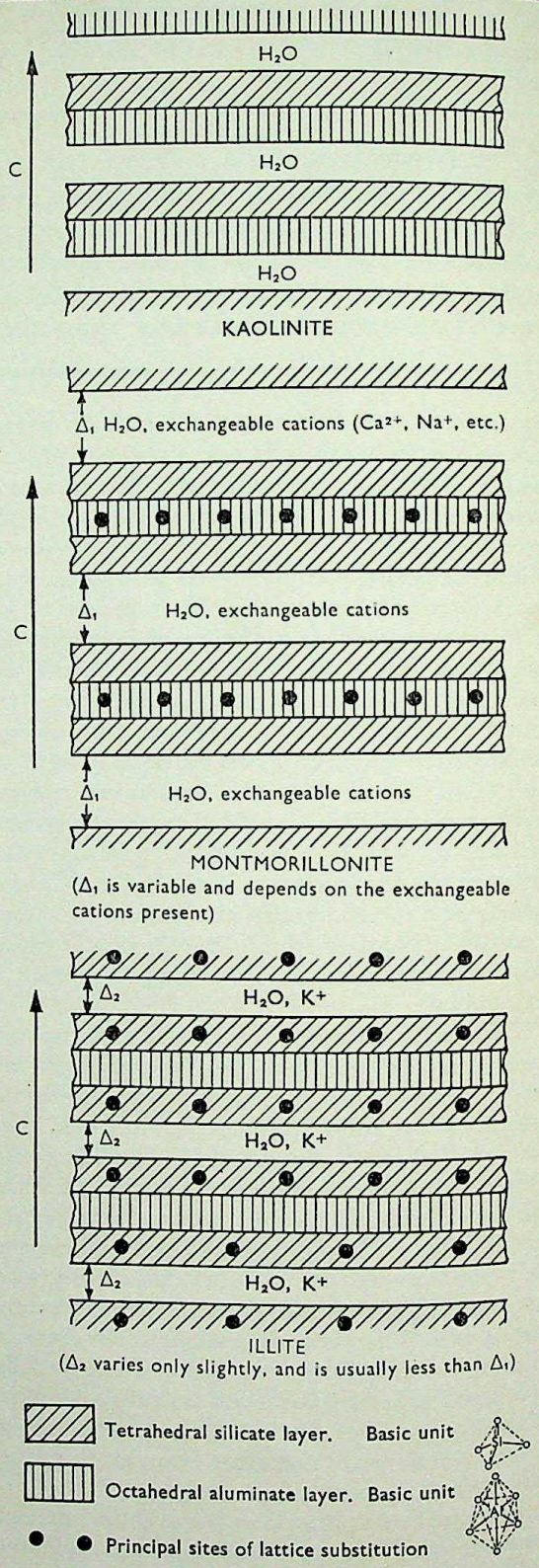

FIGURE 1 – *Diagrammatic representation of three important layer-lattice structures characteristic of certain clay minerals. Arrow indicates c-axis.*

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little replacement of Si or of Al by other ions within the layers, and consequently no other ions are necessary in the structure to balance any charge variations. Forces between the unit layers are very weak along the c-axis, and the structural formula is simple. Montmorillonite consists of triple-layer units, with the aluminate layer sandwiched between two silicate layers, and the idealized structural formula $Si_8Al_4O_{20}(OH)_4.nH_2O$. In practice a range of minerals of this type exists, in which there is substitution within the octahedral layer (of Mg^{2+}, Fe^{2+}, etc., for Al^{3+}) and to a much less extent within the silicate layer also (Al^{3+} for Si^{4+}). The net negative charge thus set up is balanced in part by the entry of cations into the interlayer spacings along the c-axis. The units are thus bound by electrostatic forces, and although the binding is stronger than in the case of kaolinite it is not sufficiently strong to prevent swelling when the material is immersed in water; cation exchange then takes place between the ions present between the layers in the crystal lattice and those entering in the aqueous solution. Montmorillonite swells considerably in aqueous solutions, the value of Δ_1 (figure 1) varying with the water content and with the degree of hydration of the cation. The extent of substitution may be judged from the statistical structural formula of a typical dioctahedral montmorillonite ($Si_8(Al_{3\cdot34}Mg_{0\cdot66})O_{20}(OH)_4.nH_2O$ + exchangeable cations); it will be noted that the sum of the numbers of atoms of aluminium and magnesium is four.

The micaceous clay minerals (illite, muscovite) have basically the same structure as the montmorillonite group, except that substitution is almost entirely in the tetrahedral layer (Al^{3+} for Si^{4+}), and the balancing cation is largely potassium. Interlayer forces are much stronger than in the montmorillonites because of the shorter distances involved; consequently swelling is limited and exchange is more difficult. Muscovite has the structural formula $K_2(Si_6Al_2)Al_4O_{20}(OH)_4$; this is basically similar to that for montmorillonite, except that some silicon has been replaced by aluminium in the tetrahedral layer, with the introduction of an equivalent number of potassium ions to balance the negative charges produced.

Cation exchange may result from either of two mechanisms, viz.:

1. *'Edge and corner' exchange.* At the edges and corners of the layers there may be broken bonds or hydroxyl groups which represent positions available for the substitution of cations; the capacity will generally be low. All clay minerals display this behaviour: for some, e.g. kaolinite, it is the sole source of cation exchange.

2. *Interlayer exchange.* In this case the increased net negative charge due to substitution in the lattice is balanced by the entry of other cations into the interlayer spaces; the extent to which this is possible depends upon the size of the entering cation and the strength of the bonding between the layers. If bonding is strong, replacement of interlayer cations by exchange is difficult, and in such cases the resulting capacity may only be comparable with that due to edges and corners, as for example in illite. In clays having weaker interlayer bonding, e.g. montmorillonite, exchange is facilitated by the ease with which water enters between the layers on immersion in a solution, so that ions are free to move and exchange within the interlayer spaces. Such clays, in contrast to the other types, swell markedly in water and aqueous solutions, and X-ray diffraction patterns show a characteristic increase in the c-axis spacing between the layers, particularly when large, highly hydrated ions such as sodium enter the structure. In clays of this type the bulk of the cation exchange capacity arises in this way.

The different sources of cation exchange in clays affect not only the capacity, but also the rates and equilibria in different clays. Rates of exchange are generally greatest for clays such as kaolinite, where only the 'superficial surface' is involved, and least on the non-expanding interlayer exchangers, where appreciable time is required to penetrate into the pores and interlayer spaces. Similarly, the affinity series for the latter will depend much more upon the size of the ion and upon the pore size of the mineral, and some ions may be excluded on steric grounds. Furthermore, while particle size has little effect upon the capacity of montmorillonite, where superficial surface exchange plays only a minor role, the capacity of kaolinite is increased fivefold on reduction of the particle size from the 10–20 μ range to that of 0·1–0·05 μ; non-expanding interlayer exchangers occupy an intermediate position, as would be expected.

Although ion exchange reactions on clays are in general reversible, there are two important exceptions to this behaviour, viz. hydrogen exchange and cation fixation. Although clays may be titrated with either acid or alkali to give typical *p*H curves, it is impossible to replace all of the exchangeable cations by H^+ ion without partially destroying the structure by leaching out Al^{3+} ion from the layers into the interlayer spaces, where

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it blocks exchange positions and leads to a reduction in capacity. Hydrogen ion exchange is thus not completely reversible, and this behaviour is most marked in the expanding interlayer exchangers. With sufficiently high acid concentrations the clay breaks down completely with solution of alumina and precipitation of silica. Physical destruction may also result from too high a concentration of Na^+ ions, owing to the high degree of swelling; deflocculation results, and the clay forms a colloidal suspension or solution. Sodium clays are generally difficult to dry and are in many cases thixotropic; this property is utilized in the employment of certain clays (e.g. Wyoming bentonite, a sodium montmorillonite) as a drilling 'mud'.

In cation fixation, a fraction of the exchange is found to be irreversible for certain ions and certain clay minerals; a notable example of this behaviour is the 'fixation' of potassium by certain soils. Although the cause is not completely understood, it is probable that certain cations of suitable size can migrate from the interlayer spaces into positions within the lattice itself, where the forces acting upon them will be much stronger. Other well-established cases include K^+ ion on illite, and the ions K^+, NH_4^+, Rb^+, and Cs^+ on vermiculite [4].

THE EFFECT OF HEATING UPON CAPACITY

Water plays an important role in ion exchange in the expanding interlayer exchangers, and we would therefore expect marked changes in capacity to occur on heating. When heated to high temperatures, water is lost irreversibly from hydroxyl groups within the structure; the interlayer spacing and the cation exchange capacity decrease together. If the temperature is raised sufficiently, the cations migrate into the lattice itself, where they are firmly held against further exchange in a mullite-type structure which is no longer free to swell and allow exchanging cations to enter. The temperature at which these changes occur depends upon the cation originally present (figure 2), and there is a close correlation between water loss and decrease in capacity. The smaller cations are free to move inside the lattice at lower temperatures than are the larger ones, and consequently Li^+ ion is 'fixed' at a lower temperature than is Na^+ ion; the non-expanding mineral illite shows a much less marked change on heating.

ANION EXCHANGE IN CLAYS

Anion exchange, which is generally of less importance, may arise in two ways. Firstly,

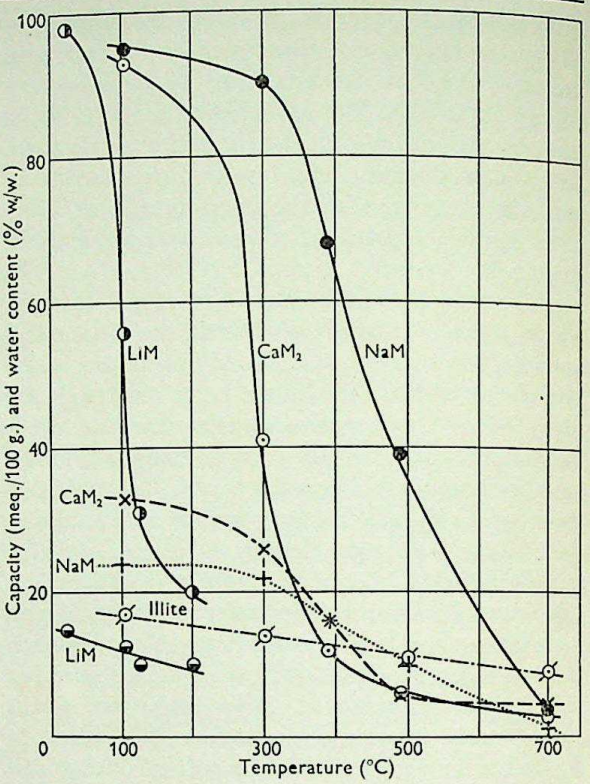

FIGURE 2 – *Water loss and decrease in capacity on heating.* ◑, ○, ●, ⌀ *Capacity*; ◒, ×, + *Water content* (M *is montmorillonite*)

surface hydroxyl groups may be replaced by other anions; secondly, the layers may grow by incorporation of other anions of suitable geometry. Thus phosphate, arsenate, and borate ions, all of which are tetrahedral, may be added to the edges of the tetrahedral silicate layers; ions such as nitrate, chloride, or sulphate are not exchanged. Anion exchange is thus essentially an 'edge and corner' reaction; consequently we find that in kaolinite the anion and cation exchange capacities are comparable, while in montmorillonite the cation exchange capacity is greatly predominant. In the case of the phosphate ion other non-exchange mechanisms may participate, providing an increased uptake of this important plant food. Thus, in neutral and mildly acid soils it will be precipitated by the trivalent oxides (Fe_2O_3, Al_2O_3) which are always present in clays, while in alkaline soils it will be precipitated as basic phosphates of the alkaline earths (e.g. Ca^{2+} in calcareous soils).

THERMODYNAMICS OF ION EXCHANGE ON CLAYS [5].

The equilibrium between two ions and a clay exchanger may be represented quantitatively by

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a typical mass-action expression. Thus in the exchange between caesium and sodium ions on montmorillonite (represented by M)

$$CsM + Na^+ \rightleftharpoons Cs^+ + NaM$$

the thermodynamic equilibrium constant K is given by the expression

$$\frac{[\overline{Na^+}][Cs^+]}{[Na^+][\overline{Cs^+}]}$$

where $[X^+]$ represents the activity of the ion X^+ in solution and $[\overline{X^+}]$ the activity in the solid phase. In dilute solutions the activities may be replaced by molar concentrations for univalent-univalent exchanges, and appropriate corrections may be made in other cases, but values of the activity coefficients in the solid phase are uncertain, and exact thermodynamic data are difficult to obtain. Until recently it has been difficult to obtain reproducible data on clay exchange equilibria, but by using standard reference clay minerals of known composition (provided by the American Petroleum Institute) H. C. Thomas has developed a sound and self-consistent treatment of the subject. Several systems have been studied on montmorillonite and on attapulgite, and Table II illustrates how the affinity series for alkali metals on attapulgite has been given quantitative meaning. The possibility of extending this treatment to the mixed clay systems found in soils is indicated by further work at Harwell [6]; an example of isotherms obtained in this work is given in figure 3.

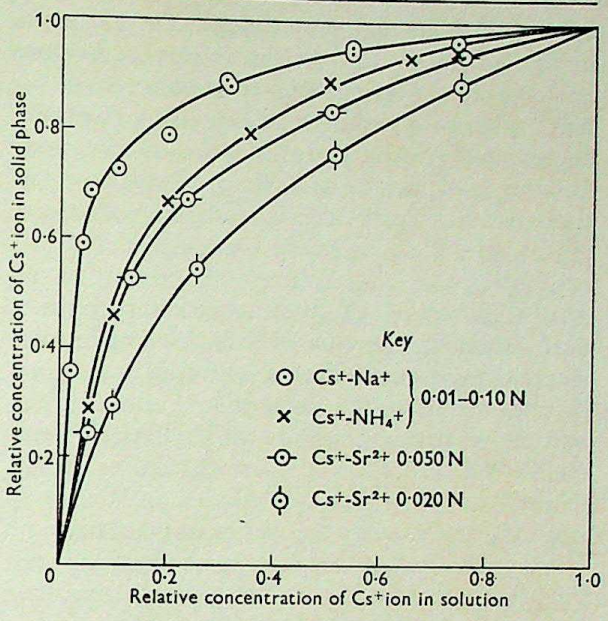

FIGURE 3 – *Representative exchange isotherms on Lower Greensand soil.*

SOME RECENT APPLICATIONS

The uptake and fixation of cations by clays and soils has acquired a new importance in connection with the treatment and disposal of radioactive wastes from the atomic energy industry. This in turn has stimulated a renewed interest in the problem of the movement of ions through the soil, and the subject has thus returned to its starting-point of more than a century ago.

Radioactivity may enter the soil either deliberately as a result of waste disposal or as fallout from a nuclear explosion, or accidentally by leakage of radioactive solutions or from a reactor accident. The activity may be either partly or wholly in soluble ionic form, depending upon its origin and upon the chemical properties of the soil. Ion exchange within the soil can play an important role in delaying the forward movement of dissolved activity and so restricting the spread of contamination. An obvious corollary to this behaviour is that the activity will remain concentrated in a limited area, within which it is readily available to plants and hence also to animals and to man unless preventive steps are taken.

Use has been made of this property of the soil for the 'ground disposal' or 'ground storage' of radioactive waste solutions at certain American atomic energy sites which are situated in relatively unpopulated areas where geological and climatic conditions are also favourable [7]. Even in the vast areas of the American continent indiscriminate disposal is not possible, and the volumes and total activity discharged must be carefully adjusted

TABLE II

Alkali metal ion exchange equilibria on attapulgite
(Merriam and Thomas, *J. Chem. Phys.*, **24**, 993, 1956)

Equilibrium (A is attapulgite)	Thermodynamic equilibrium constant, K		Free energy change $\Delta G = -RT \ln K$	Heat of exchange ΔH (30–75° C)
	30° C	75° C		
$Li^+ + CsA \rightleftharpoons LiA + Cs^+$	0·029	0·068	2·14 kcal.	4·1 kcal.
$Na^+ + CsA \rightleftharpoons NaA + Cs^+$	0·041	0·090	1·93 (30° C)	3·8
$K^+ + CsA \rightleftharpoons KA + Cs^+$	0·131	0·243	1·22	2·9

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as a result of searching preliminary investigations in the laboratory and in the field; the ultimate criterion is the maximum permissible level for safe discharge to public water supplies. Laboratory experiments show clearly that the cations move forward much more slowly than the movement of water as measured by the rate of anion movement (figure 4); if the delay is sufficiently long the activity of the cations may be reduced to an acceptable figure. A much more complex situation obtains in the case of a mixture of cations, where the movement of each cation depends upon its concentration, the total ionic concentration, and the relative affinities of the exchange centres for the different cations. Here the results cannot be predicted, but must be evaluated in laboratory and field trials, using the wastes in question.

The affinity series for cations on clays and soils follows approximately the Hofmeister series, that is the affinity increases with increasing cationic charge and with decreasing hydrated ion radius, although there are many exceptions to this rule. On introducing a mixed waste into the ground the rates of travel are found to increase in the following order:

$$Pu^{4+} < La^{3+}, Ce^{3+}, \text{etc.} < Sr^{2+} < Cs^{+}$$

The more rapid movement of strontium and caesium, coupled with their long half-lives and the high biological toxicity of ^{90}Sr, implies that the quantity of these in the waste must be carefully controlled, and some form of chemical pretreatment may be necessary. Of the other species usually present in wastes, nitrate ion travels far ahead of the cations, with ruthenium intermediate between it and the cations. This is because, although Ru^{3+} ions behave similarly to the other trivalent ions, much of the ruthenium in waste solutions is present in complex uncharged or anionic species, which therefore travel much faster than the cations. Use is made of the faster movement of nitrate and of ruthenium in monitoring the spread of contamination; when these are detected in suitably located monitoring wells the discharge point is closed down, with the knowledge that the main activity front may be up to two years distant relative to the rate of movement through the soil. The failure of soils to retain nitrate ions can itself have severe consequences owing to their toxicity in high concentrations; several fatalities among the turtle population at Oak Ridge have been traced to this cause.

If the composition of the waste solution being discharged at one point is altered, this may result

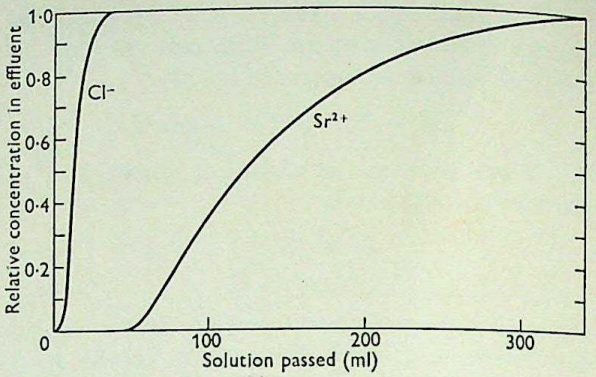

FIGURE 4 – *Breakthrough of Sr^{2+} and of Cl^{-} ions on passage through soil columns* (*Kaufman, W. J.*, et al., *Tech. Inf. Div., Oak Ridge, TID-7517, 207, 1956*).

in an accelerated movement of the ions already exchanged, particularly if the acidity is increased. In this case also the relative rates of movement of cations may be altered, since some are more easily displaced by hydrogen ions than are others (figure 5); strontium is particularly susceptible, an important fact in view of its toxicity. High concentrations of inactive salts will also reduce the uptake of activity by competing for the exchange sites. These facts emphasize the dynamic nature of this method of waste disposal and underline the need for careful evaluation of methods.

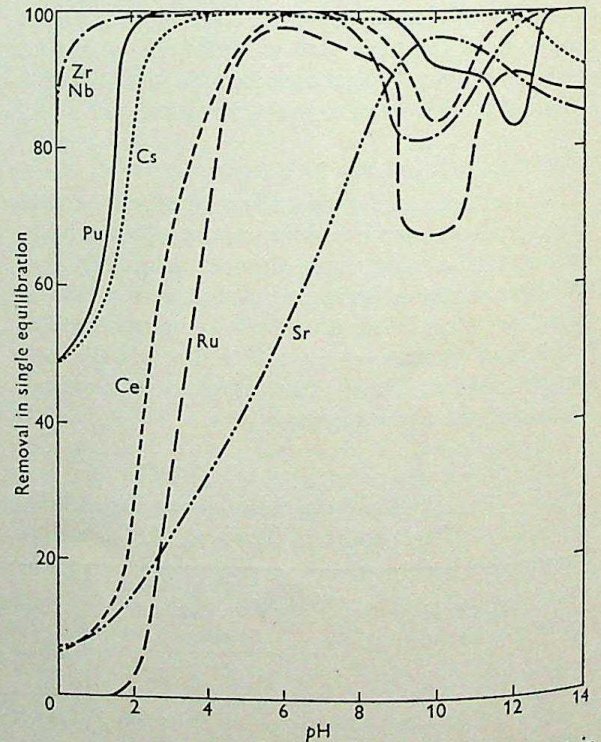

FIGURE 5 – *Effect of acidity on the uptake of certain cations by Hanford, Washington, soil* [7].

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TABLE III

Removal of activity by slurrying with clay [8][1]

Activity	Ionic form	*p*H	% removal			
			p.p.m. clay 1000	2000	3000	4000
^{90}Sr–^{90}Y	Cationic	7·7	83·4	89·1	92·9	95·0
^{140}Ba–^{140}La	Cationic	7·8	88·8	92·0	94·3	97·1
$^{141,144}Ce$–^{144}Pr	Cationic	8·0	99·7	99·8	99·9	99·9
^{95}Zr–^{95}Nb	Cationic and colloidal	7·5	98·0	99·1	99·4	99·6
^{106}Ru–^{106}Rh	Mixed	5·2	50·5	59·3	61·5	—
Aged fission products	Mixed	8·8	82·0	82·9	86·3	90·3
^{131}I	Anionic	7·5	4·9	5·0	3·4	3·4

[1] Local shale from Oak Ridge; capacity 29 meq./100 g.

It has been suggested that natural ion exchangers should be used as a final stage in the treatment of low-activity effluents, in order to remove ions such as Cs^+ which are not affected by conventional precipitation and flocculation treatment. Although soils of suitable capacity are plentiful, they cannot be used in large beds with the flow rates required. The mineral vermiculite has, however, excellent porosity and packing properties combined with a useful capacity (table 1); laboratory trials have demonstrated good decontamination factors, and larger-scale trials are now in progress at Harwell.

The decontamination of water by batch slurrying with clay has been proposed for the treatment of contaminated water supplies in the field [8]. This method would be unsuitable for the routine treatment of radioactive effluent, since the quantities of clay required would lead to undesirably large volumes of active sludge requiring dewatering and disposal. Removals obtained in single batches are given in Table III. In an emergency, sufficient reduction of activity could also be obtained by percolation through a column of soil.

The use of clay has also been suggested [9] for disposal of highly active waste, by absorbing the ions and then fixing them against leaching by firing to temperatures of ~ 1000° C. Advantage is taken of the ease with which ion-exchange operations can be carried out by remote control, an essential requisite for processing of highly active materials. The original liquid waste is converted into a solid form, which seems preferable for the centuries-long storage required for the decay to negligible levels of ^{137}Cs and of ^{90}Sr. This process has been successfully developed on a pilot plant scale both at Harwell and at Brookhaven, although it is doubtful whether it will be adopted on a larger scale except possibly for relatively pure active species free from inactive diluents.

In conclusion, it is obvious that the data obtained in relation to waste disposal, and the fundamental studies which are being carried out in order to provide a background for this work, cannot fail to be of interest to all whose work touches on this subject. The movement and retention of ions within the soil, the concentration of trace elements by ion exchange, and the equilibria between the soil constituents and the ions dissolved in the soil water are all of fundamental importance to workers in many fields.

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Chloroplast structure and function

J. B. THOMAS

The achieving by man of a reasonable efficiency in the conversion and chemical storage of solar energy would be an event of major importance. The study of the mechanism of photosynthesis, in which this problem is naturally solved, should give information that would help towards this achievement. By combining electron microscopy with other physical and chemical techniques considerable progress has been made in localizing the photosynthetic centres of the plant cell. These methods and some of the results are reviewed here.

Attempts to utilize solar energy with reasonable efficiency by converting and storing it chemically meet with the difficulty of the rather quick loss of the excitation energy. Therefore the main problem concerns energy storage rather than energy conversion. E. I. Rabinowitch [1] pointed out that transfer of an electron from a molecule excited by the absorption of light energy to an atomic nucleus of a different molecule offers the best chance for energy storage, provided the electron affinity of the 'acceptor' is much lower than that of the 'donor'. In this way there is sufficient time for enzymatic stabilization or spatial separation of the primary products. In other words, oxidation-reduction reactions against the electrochemical potential gradient are suitable for providing conditions required for the chemical storage of light energy.

Evidently nature has solved the problems in question quite satisfactorily. Solar energy is converted into chemical energy by the reduction of carbon dioxide to carbohydrates with the help of 'photosynthetic' pigments. Therefore a study of the natural photosynthetic apparatus may provide some insight into the reason why the process, or rather chain of processes, under consideration proceeds with a reasonable efficiency in the living cell.

The action spectrum of photosynthesis shows maxima and minima corresponding to those of the absorption spectrum of chlorophyll. It is therefore clear that this pigment absorbs photosynthetically active light. Moreover, the occurrence of additional maxima in the action spectrum demonstrates that light absorbed by carotenoids and, in red and blue-green algae also by phycobilins, is utilized in photosynthesis too. A detailed discussion of the pigments is beyond the scope of the present review, but may be found, with other more detailed information about photosynthesis, in the excellent book of Rabinowitch [2]. It will suffice to mention that in higher plants two chlorophylls occur, chlorophylls *a* and *b*, and that the ratio *a* : *b* is about 3 : 1. Photosynthetic bacteria contain related compounds: bacteriochlorophyll (purple bacteria) and bacterioviridin (green sulphur bacteria).

Excited chlorophyll molecules release their excitation energy in three ways: as heat, fluorescence, and chemical energy. Since the conversions are random processes, fluorescence can be used as a quantitative indicator of the excitation energy, and this has been done by many authors. The most detailed quantitative study on this subject is that of L. N. M. Duysens [3]. If green plants are irradiated, only chlorophyll *a* fluorescence is observed. Calculation showed that excitation energy of light absorbed mainly by the carotenoids is transferred to chlorophyll *a*. The efficiency of this transfer, however, is relatively low (20–40 per cent). Previously H. J. Dutton, W. M. Manning, and B. M. Duggar [4] and E. C. Wassink and J. A. H. Kersten [5], independently of each other, had found with diatoms that light energy absorbed by the carotenoid fucoxanthin is as active in photosynthesis as light absorbed by chlorophyll. Fluorescence experiments showed further that energy absorbed by fucoxanthin enters the chain of chemical 'dark' processes in photosynthesis through chlorophyll. In this case a transfer of excitation energy occurs with an efficiency approaching 100 per cent. Others found that the same is true of such transfers from phycobilins to chlorophyll as well as from chlorophyll *b* to chlorophyll *a*, or, in purple bacteria, from bacteriochlorophyll types 'B800' and 'B850' to type 'B890'. The occurrence of such high efficiencies suggests that the transfer mechanism involves inductive resonance [6]. It may be added that the low-efficiency transfers first mentioned can be readily explained by the fact that the 'overlap' of the spectra of the carotenoids in question with that

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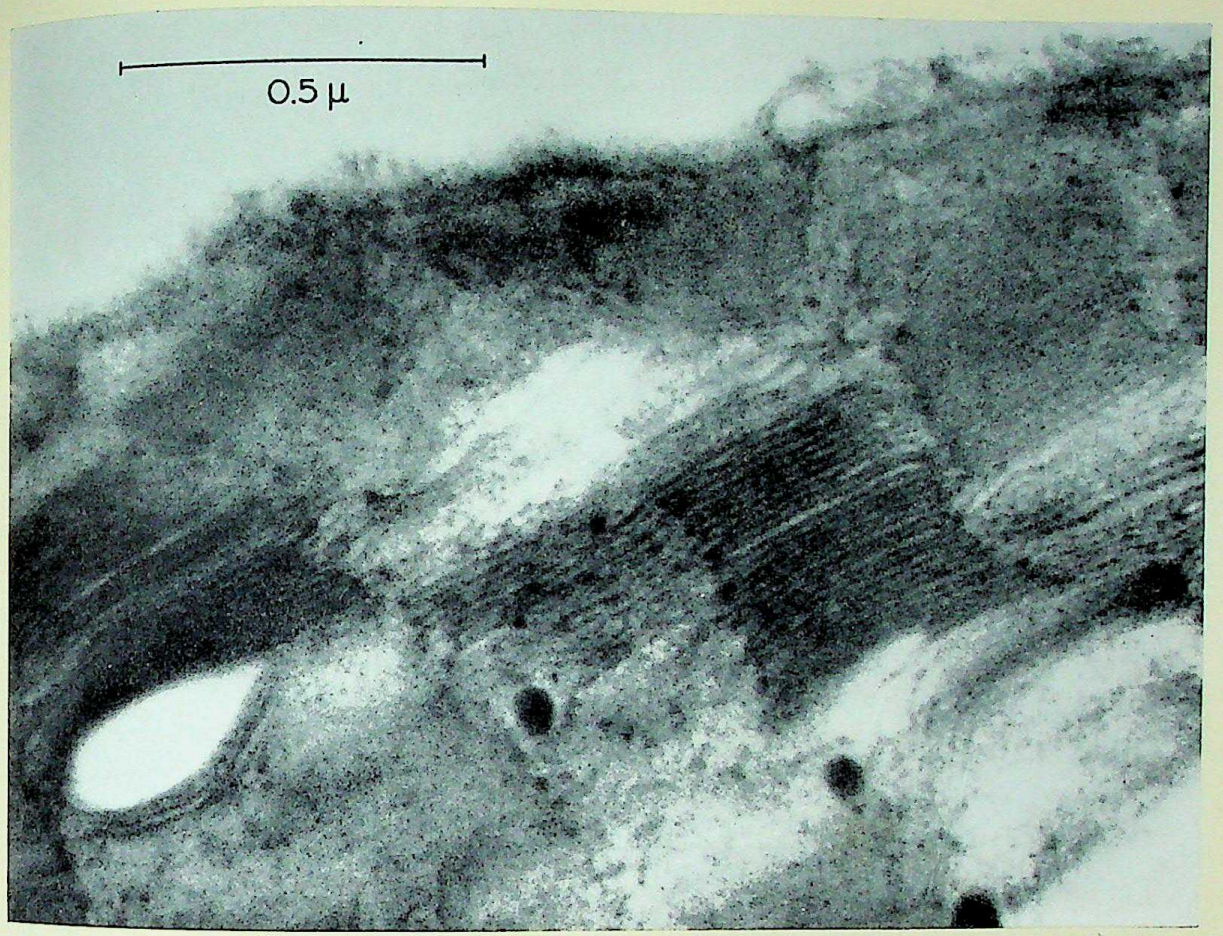

FIGURE 1 – *Detail of chloroplast of* Elodea densa. (*Electron micrograph by courtesy of P. F. Elbers.*)

of chlorophyll is poor, whereas in the other cases mentioned a good overlap occurs.

Apart from the spectral requirements, pigment concentration must be sufficiently high for efficient energy transfer to occur, and the transfer will be facilitated by suitable orientation of the pigment molecules. Hence the state in which the pigments occur in the living cell is of primary importance for their functioning. The data concerning this state will be briefly discussed first.

If pigments are extracted by grinding the photosynthetic cells in water, the absorption spectrum of the colloidal extract is the same as that of the intact cells. However, extraction with organic solvents, including alcohols, ether, acetone and others, yields a clear solution and the absorption maxima are shifted as compared with those of the spectrum *in vivo*. This proves that the state of these pigments in the living cell differs from that in solution.

This difference of state is also demonstrated by the fact that chlorophyll fluorescence is much weaker *in vivo* than in solution. It is indeed most curious that chlorophyll fluoresces at all in the natural state. If a solution of this pigment in an organic solvent is prepared of the same concentration as occurs in the chloroplast (about 0·2 M), concentration quenching will extinguish any observable fluorescence. It is also observed that a colloidal dispersion of chlorophyll in water is non-fluorescent. Why chlorophyll in the aqueous medium of the cell is capable of fluorescence remains to be explained.

For various reasons photosynthetic pigments in their natural state seem to be bound to proteins, or rather lipoproteins. One of the functions of these protein-pigment complexes is evidently the establishment of conditions required for the functioning of the pigments, e.g. by establishing some definite spatial arrangement. As a rule, the protein-pigment complexes are incorporated in distinct cell structures, the chloroplasts. The

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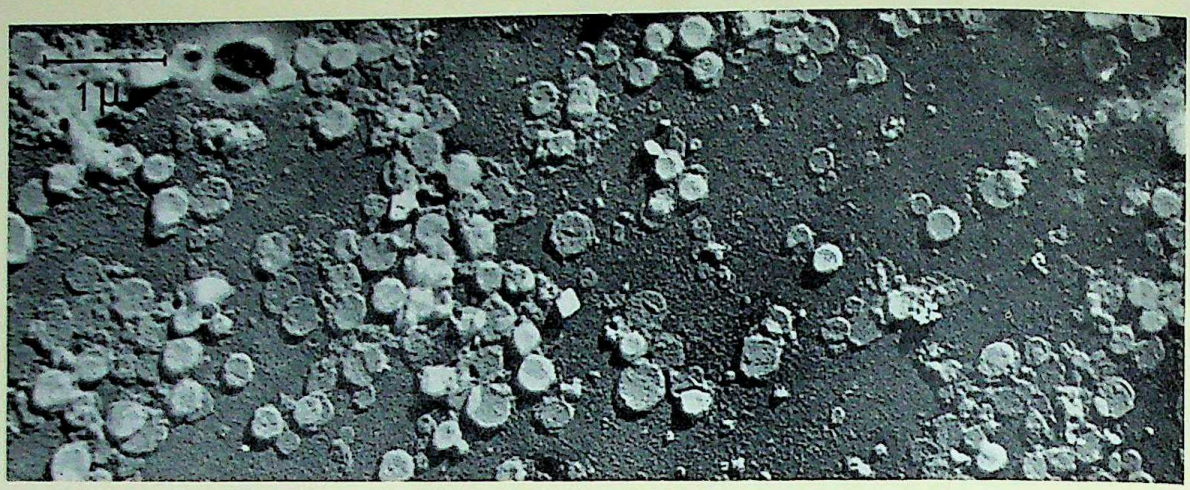

FIGURE 2 – *Electron micrograph showing isolated whole and fragmented grana from chloroplasts of* Spinacia oleracea.

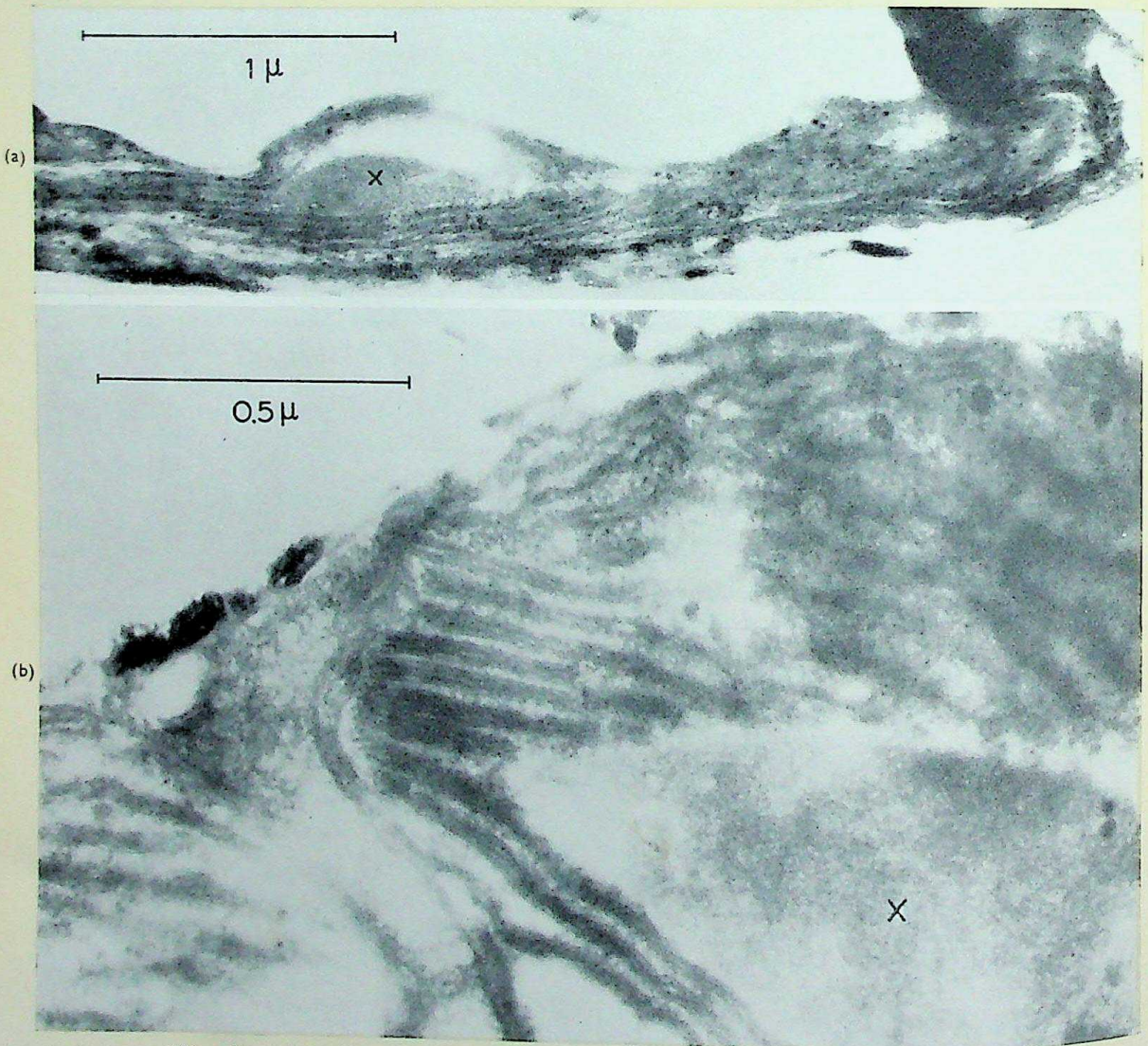

FIGURE 3 – *Chloroplast of the diatom* Nitzschia dissipata. (a) *General view.* (b) *Detail. Product of photosynthesis denoted* x *in both. (Electron micrograph by courtesy of P. F. Elbers.)*

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structure of these organelles may be discussed first.

Chloroplasts contain lamellae, of a thickness of roughly 50 Å, embedded in a globular material, the stroma, and are most probably surrounded by a double membrane with semi-permeable properties. Two main types can be distinguished: 'granular' chloroplasts, which seem to be typical of higher plants, and 'lamellate' ones. Figure 1 shows an electron micrograph of a section through the granular chloroplast of *Elodea densa* in which the components mentioned can be seen. In various regions the contrast as well as the compactness of the lamellae is increased. This increase in contrast is due to the fact that these regions are more heavily 'stained' by the strongly electron-scattering osmic acid during preparation than the remaining parts of the lamellae. It indicates that the dark regions are rich in lipids. If such chloroplasts are damaged after isolation from the cell, e.g. by osmotic effects, they become readily disintegrated except for these dark regions. The latter become freed from the chloroplasts and, as shown in figure 2, show up as disks. Each disk represents a stack of circular lamellae. As can be seen, some stacks are disintegrated into isolated lamellae. The lamellar stacks which, more or less, represent structural units are called 'grana'.

In the chloroplast, the grana are embedded in the 'stroma substance'. It consists of a globular proteinaceous substance through which lamellae connecting the grana are extended. These lamellae, or rather parts of the lamellae, are called 'stroma lamellae' as opposed to the 'grana lamellae'.

The lamellate chloroplasts do not contain grana. As a rule, the lamellae are evenly 'stained' with osmic acid throughout these chloroplasts, while no regions at which the lamellae seem to be relatively closely packed are observed. These chloroplasts are often found in algae. Figure 3 shows such a chloroplast of the diatom *Nitzschia dissipata*. As often found in lamellate chloroplasts, the lamellae occur in bundles of from 4 to 8. The globular stroma occurs in relatively thin layers between these bundles. Stroma lamellae are absent.

In a few cases forms intermediate between both types of chloroplasts were observed, namely in *Spirogyra* by T. Butterfasz [7] and in some Desmidiaceae by R. Chardard and C. Rouillier [8]. For reviews of studies on chloroplasts the reader may be referred to H. Leyon [9], D. von Wettstein [10], and J. B. Thomas [11].

In blue-green algae and in photosynthetic bacteria no true chloroplasts occur. In the former organisms W. Niklowitz and G. Drews [12] observed lamellae situated in the cytoplasm of the cells. Neither chloroplast membrane nor special stroma was observed. The authors call a structure such as this, which is probably photosynthetic, 'chromatoplasm'. In photosynthetic bacteria 'free' grana occur. The same authors [13] were the first to present a beautiful electron micrograph of organelles—from the purple bacterium *Rhodospirillum rubrum*—that most likely are free grana. The lamellate structure was evident. No surrounding membrane was observed.

In conclusion, it can be said that the structure of the photosynthetic apparatus may be different in different species but that lamellae occur in all. This statement strongly suggests that the presence of lamellae is a prerequisite for photosynthesis. Some data concerning these structures are therefore discussed below.

The resolving power of the light microscope is such that grana can be observed only if their diameter exceeds 0·3 μ. Examination of chloroplasts containing grana of these or larger dimensions shows that these grana are green. The surrounding stroma, however, is colourless. Moreover, under the fluorescence microscope the grana exhibit a red fluorescence, while the stroma remains dark. These observations may demonstrate that the occurrence of chlorophyll is restricted to the grana. Since in aqueous extracts the carotenoids are found always to accompany the chlorophyll-protein complexes, most probably the same can be said of these accessory pigments.

An attempt [14] to localize the chlorophyll more precisely was made as follows. The presence of chlorophyll can be demonstrated by the Molisch reaction, in which silver nitrate is reduced to metallic silver by illuminated chlorophyll. This reaction offers a possibility of locating the chlorophyll more exactly. A suspension of grana is vibrated sonically at 7000 c/s until the grana are disintegrated into lamellae. To the suspension silver nitrate is added, and the mixture is illuminated. Electron-microscopical examination shows that silver deposits are formed at the surface of the lamellae. It may therefore be assumed that the lamellae function as carriers of chlorophyll and probably also of the accessory pigments.

The same conclusion can be drawn in a less direct way from the fact that a suspension of lamellae is capable of photolysing water. As soon as granular chloroplasts are fragmented, their ability to photosynthesize is lost, but in the presence of

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a suitable hydrogen acceptor they are capable of photochemical liberation of oxygen from water. The action spectrum proves that this 'Hill reaction' is mediated by excited chlorophyll.

If such a suspension is further vibrated, the lamellae become fragmented. Even these fragments are capable of the Hill reaction. During the disintegration the reaction rate at first remains fairly constant but then drops rather quickly. This occurs at a particle volume of about 10^6 Å^3 [15], and it has been calculated that these particles contain roughly 100 chlorophyll molecules. At a volume of 2×10^5 Å^3 the ability to perform the Hill reaction is totally lost. This means that at these dimensions all chlorophyll molecules are removed from the enzymic centres at which the products of the Hill reaction are formed with the aid of the excitation energy from the chlorophyll. It also means that at a volume of about 10^6 Å^3 the enzymic centres are provided with their complete number of active pigment molecules.

A. Frey-Wyssling and E. Steinmann [16] pointed out that, in good electron micrographs, the lamellae can be seen to consist of spherical particles. The authors suggested that these particles represent protein macromolecules. It remains to be seen whether the globular shape is natural or not. During fixation of electron microscopical preparations, spirally shaped macromolecules may well curl up and attain a spherical form. Anyway, it seems very likely that the lamellae are built up of macromolecules. The dimensions of these macromolecules proved to be of the same order of magnitude as those of the smallest particles still fully active in the Hill reaction.

The above data demonstrate another function of the grana lamellae, namely the carrying of enzymic centres. However, there are strong indications that not all the photosynthetic enzymes are associated with the lamellae. This may be suggested by the fact that it was found to be possible to restore photosynthetic activity of isolated spinach chloroplasts, damaged by treatment with water to such an extent that the CO_2-fixing capacity was lost, by addition of an enzyme extract from whole chloroplasts [17]. A comparison of fluorescence induction phenomena in granular and lamellate chloroplasts [18] shows that part of the photosynthetic enzymes are probably located in the stroma.

Estimations of both total surface of grana lamellae and chlorophyll content of chloroplasts from various plant species showed that in all cases the lamellar area available per chlorophyll molecule was about the same, namely a few hundred square ångströms. Since transfer of excitation energy with an efficiency of 96 per cent between chlorophyll *a* and *b*, or between the bacteriochlorophyll types mentioned earlier, is possible if each molecule of these pigments occurs within an area of roughly 10^3 Å^2, it is evident that the area available per chlorophyll molecule is such that a highly efficient energy transfer is possible. This is also true in lamellate chloroplasts if the chlorophyll is assumed to be evenly distributed over the lamellar surface throughout the whole chloroplast. Because of the fact that the total lamellate chloroplast shows a red fluorescence under the ultra-violet microscope, it seems likely that such a homogeneous distribution of chlorophyll occurs. This conclusion is supported by the electron-microscopical observation that the total area of lamellae from this type of chloroplast is evenly stained with osmium tetroxide. With 'intermediate' chloroplasts [7] the above may be only partly true.

As the pigment-carrying lamellae are thin layers, their thickness being about 50 Å, the question arises whether the pigments occur in an orientated state. It was mentioned before that orientation may facilitate energy transfer. Earlier investigations, in which dichroism was measured, yielded contradictory results. J. C. Goedheer [19], however, pointed out why measuring of dichroism is not suitable for studying pigment orientation in these particular objects. He studied dichroism (anisotropy of absorption) in combination with birefringence (anisotropy of refractive index) and polarization of fluorescence by chlorophylls both *in vitro* and *in vivo*. These experiments led to the following conclusions. First, in the chloroplast the chlorophyll molecules are probably concentrated in thin films, occupying less than 7 per cent of the cross-section of a lamellate chloroplast or granum. Second, combination of experiments both *in vitro* and *in vivo* showed that, most probably, the oscillator responsible for the red absorption band of chlorophyll has a preferred orientation in the direction of the lamellae. The same is true of the 'infra-red oscillator' of bacteriochlorophyll. As, moreover, experimental evidence was obtained that the oscillators in question are orientated in the direction of the shortest dimension of the conjugated ring system which constitutes the porphyrin 'head' of the chlorophyll and bacteriochlorophyll molecules, it could be concluded that, most probably, the porphyrin rings of these

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pigments are preferentially orientated in the plane of the lamellae.

The orientation, however, was found to be relatively poor. As an explanation for this phenomenon Goedheer suggested that the pigment molecules may be arranged at the 'curved' surface of the earlier-mentioned spherical or spirally shaped lipoprotein macromolecules.

In grana or in lamellate chloroplasts the lipoprotein layers, the lamellae, alternate with layers of 'free' oriented lipids. Experiments on staining with rhodamine B suggested that the carotenoids are not arranged among these free lipids. Thus they probably are located, together with the chlorophyll molecules, at the surface of the lipoprotein layers.

Summarizing the available experimental evidence, it can be said that the lamellae of grana and lamellate chloroplasts are carriers of the photosynthetic pigments and, as such:

(1) make possible the functioning of the photosynthetic pigments in the aqueous medium of the cell by providing the required complex of photosynthetic pigments with their associated lipoproteins;

(2) ensure the maintenance of intermolecular distances at which an efficient energy transfer is possible. This may be important, because changes in the water content of the chloroplasts may occur during photosynthetic formation of osmotic substances. Thus the intermolecular distances of the pigments might considerably increase if these substances occurred in some more or less 'dissolved' state. Such a situation might be imagined to occur if the pigments were rendered soluble, e.g. by combination with some other substance;

(3) may facilitate energy transfer by orientation of the photosynthetic pigments; and

(4) are carriers of enzymic centres involved in the first steps of the photosynthetic reaction chain and thus guarantee a close connection between these enzymic centres and chlorophyll molecules under possibly varying conditions (*cf.* under 2).

Another function of the lamellae was suggested by Leyon [9] and von Wettstein [10]. The occurrence of more or less closely packed lamellae may give rise to the occurrence of flattened capillary channels extending throughout the chloroplast. Along the walls of these channels the photosynthetic products may be transported quickly. It certainly would be worth while to try to get experimental confirmation of this hypothesis.

With regard to the problem initially mentioned, concerning the way in which excitation energy is efficiently stored in the photosynthetic apparatus, the above considerations suggest that the lamellar structures discussed may provide at least two essential requirements for this process: firstly, adequate intermolecular distances for an efficient energy transfer down to some enzymic centre are maintained, and, secondly, the first photoproducts may be quickly transported and thus may be prevented from recombining. However, much more research is needed to settle this question definitely.

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Book reviews

EUCLIDEAN GEOMETRY

Problems of Euclidean Space, *by H. G. Eggleston. Pp. viii + 165. Pergamon Press Ltd, London.* 1957. 40*s. net.*

This short, expensive, and extremely interesting book was the Adams Prize Essay of the University of Cambridge for the year 1955–56. It gives an admirable account of the concept of convexity in contemporary mathematical research. Typical problems which are studied in detail are geometrical properties for which triangles are the extremal convex curves, and the asymmetry of curves of constant width. In the early chapters the problems are of a general nature, and the major difficulty is that of conjecturing the correct result. In the last chapter the correct result can often be guessed without difficulty by those who are expert at the subject, but the real difficulty is then to establish the truth of the conjecture.

The problems of convexity are developed from several distinct points of view: (1) the consequences of the fact that a closed convex set is an intersection of closed half-spaces; (2) the behaviour of convex sets as particular types of connected sets; (3) the discovery of extremal properties for particular convex sets; (4) the relation between simple geometrical figures and general convex sets as a direct approximation; and (5) the discovery of problems in which convex sets in two or three dimensions are the appropriate generalizations of intervals in one dimension.

The treatment throughout is very sophisticated but very rewarding, and this little book will be of the utmost value to those who wish to learn the results of the most recent research in this fascinating branch of modern pure mathematics.

G. TEMPLE

STATISTICAL MECHANICS OF FLUIDS

Mécanique statistique des fluides, *by D. Massignon. Pp. xiv + 264. Dunod, Paris.* 1958. *Fcs* 3900 *net.*

This book is sponsored by the French National Council of Scientific Research and is part of the mathematical series directed by Professor A. Lichnerowicz of the Collège de France. It uses elaborate vectorial writing and is a systematic account of general formulations common to all statistical theories of more or less condensed fluids; perfect gases or critical states are studied only as special cases.

Local measurements, even so-called instantaneous ones, give only mean values for systems of particles subject to thermal agitation, values that are more or less refined or loaded in favour of the point and moment aimed at. Using the essential notions of dispersion and correlation, hydrodynamic values are correctly defined in Euler variables, and lead to the density, pressure, and mass stream fluctuations corresponding to fluid temperature and thermodynamic characteristics. Such fluctuations, while generally not directly accessible, increase near the critical point.

Quantic hydrodynamics and statistics are developed in the last chapter for boson and fermion systems so as to explain the large critical volume and low critical temperature of hydrogen and helium.

Appended definitions, formulae, and references make things easier even for readers unaccustomed to statistical mechanics.

E. S. TRAVERS

ELECTRON IMPACT PHENOMENA

Electron Impact Phenomena and the Properties of Gaseous Ions, *by F. H. Field and J. H. Franklin. Pp. ix + 349. Academic Press Inc., New York; Academic Books Ltd, London.* 1957. $8.50 *net.*

This book contains an extensive and critical compilation of data on electron impact phenomena. It reviews the properties of gaseous ions with particular reference to the chemical physics of the systems studied. The recent increase in the amount and variety of information in this field has been largely due to the availability of commercial mass spectrometers, and a major part of the book is concerned with the results of such studies. However, the treatment is general and is concerned with the tabulation and interpretation of all available energetic data pertaining to electron impact phenomena. The importance of such data in connection with molecular structure and chemical behaviour is stressed and recent theoretical treatments of ionization potentials and fragmentation processes are given. A chapter is devoted to the relevance of the ionization potentials of gaseous organic molecules to the mechanisms of reactions in solution which proceed through ions or ion pairs. Attention is also called to the possible significance of this type of data in radiation chemistry and catalysis. The book serves to summarize and collate the information in a field of growing importance.

W. C. PRICE

CHEMICAL THERMODYNAMICS

An Introduction to Chemical Thermodynamics, *by E. F. Caldin. Pp. xv + 424. Clarendon Press, Oxford.* 1958. 50*s. net.*

This is a very straightforward introduction to chemical thermodynamics, based on the Gibbs method. This involves introducing the basic equation of Gibbs, viz. $dE = dq + dw + \mu_1 dn_1$, on page 29. This leads at once to the definition of chemical potentials, but the condition $dE = \mu_1 dn_1$, in which a substance is added without gain of heat and with no work done, is somewhat difficult to realize. I think that students who are not very analytically minded may have difficulty here if they have no previous knowledge of the subject. However, granted this approach, the subject is developed very clearly and logically, and a reasonable amount of illustrative data is given. Every author has difficulties with arrangement, and often what is gained in one direction is lost in another. At the same time it seems a little difficult to deal with galvanic cells as a means of determining standard potentials of ions on pages 252–68, when the activity coefficients of electrolytes in solution are not reached until the final very short chapter of only twenty pages, at page 386. The price fixed by the publishers is disturbingly high for what is obviously intended as a students' textbook.

J. A. V. BUTLER

RADIOISOTOPE LABORATORY TECHNIQUES

Radioisotope Laboratory Techniques, *by R. A. Faires and B. H. Parks. Pp. xii + 244. George Newnes Ltd, London.* 1957. 25*s. net.*

This book briefly describes the kinds of laboratory, apparatus, and technique required by the use of radioactive isotopes. The first chapters deal in an elementary way with the production and properties of radioactive isotopes, accommodation and equipment needed for their safe manipulation, and the instrumentation required for their detection and assay. The final chapters give an idea of how radioactive isotopes

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may be used as tracers and as sources of ionizing and penetrating radiation. Appendices provide data such as count-rate losses for the single paralysis time of 400 μsec, an energy-range curve for mono-energetic electrons, and a selected list of pile-produced radioactive isotopes and their properties.

The book is varied in quality. On one hand it contains some useful and up to date practical information on health hazards, laboratory accommodation, and manipulation techniques; thus a novel and useful demountable filter is described for the collection of geometrically uniform precipitates. On the other hand, the treatment of principles is inadequate. For example, the description of radioactivation analysis (p. 198) is very misleading on account of the unfortunate use of the term 'constituent' where 'element' would have been better. The book would have been greatly improved by a little more care in editing. It is recommended not as a general introduction to radioisotope techniques but as a useful book for the intelligent worker who has a clear idea of how radioisotopes may be helpful but who is not clear about the purely practical applications in the laboratory. F. P. W. WINTERINGHAM

NATURAL QUINONES

Naturally Occurring Quinones, *by R. H. Thomson. Pp. vii + 302. Academic Press Inc., New York; Butterworths Scientific Publications, London.* 1957. 50*s. net.*

The last comprehensive account of the quinones occupies some 59 pages in Mayer and Cook's 'The Chemistry of Natural Colouring Matters' published in 1943. The present volume is an indication of the rapid extension of our knowledge: it describes over 150 natural quinones isolated from plants, fungi, lichens, insects, and sea urchins, ranging in complexity from *p*-benzoquinone to the nonacyclic erythroaphins. Although the majority of these quinones contribute little to natural colouring, this is not always the case; e.g. the anthraquinone parietin (physcion) of the common yellow-orange lichen, *Xanthoria parietina*, picturesquely decorates rocks, walls and roofs in country and seaside districts in many countries.

The chapters deal with functions and biosynthesis; benzoquinones, including terphenyl- and dibenzoquinones; the large group of naphthaquinones; the anthraquinones, which include many long-known compounds used as dyes, and certain insect pigments; the few phenanthraquinones; and a final chapter on the aphins and on the photodynamic pigment hypericin. There are some 1000 references to the original literature, a good general index, and also zoological and botanical indexes.

This is a stimulating, well-written and well-printed book which is much more than a catalogue of structures. The bright orange of the dust cover seems particularly appropriately chosen, and one is tempted to see if it can be reduced to a colourless dihydro form.

W. BAKER

PHENAZINES

Phenazines, *by G. A. Swan and D. G. I. Felton. Pp. xix + 693. Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1957. $22.50 *net.*

This is the eleventh volume in the series of monographs 'The Chemistry of Heterocyclic Compounds' edited by A. Weissberger, and it maintains the high standard set by the previous numbers. Part I, by G. A. Swan, deals with phenazines (9:10-diaza-anthracenes) which are not fused to other ring systems, and reviews their general methods of synthesis, their homologues, hydrogenated derivatives, and substitution products, with special sections devoted to hydroxy, keto, and amino derivatives. Separate chapters then follow on the safranines, the indulines and nigrosines, aniline black, the bacterial pigments derived from the phenazine nucleus, and biological properties.

Part II, by D. G. I. Felton, summarizes existing knowledge of monobenzophenazines and the related phenazinium salts and their hydroxy and amino derivatives, penta- and polycyclic phenazines, indanthrone, and phenazines which are condensed with other heterocyclic systems.

This is essentially a reference book, and each chapter has a very full bibliography; there is a comprehensive subject index. Nevertheless it is a book which can be read with pleasure, not only because of the excellence of the writing but also because of the attractive graphic formulae which are used with generosity. The authors have performed a very valuable service in condensing the extensive and diffuse literature of the phenazines into an orderly and logical review. W. BAKER

CRYSTALLOGRAPHY

Structure Reports for 1951, *Vol. XV, edited by A. J. C. Wilson. Pp. viii + 588. International Union of Crystallography. Oosthoek's Uitgevers Mij., Utrecht.* 1957. £10 10*s. net.*

This volume, which is profusely and excellently illustrated, gives details of the crystal structures of some 2000 metals, inorganic and organic compounds, analysed by means of X-ray or neutron-diffraction methods and published during 1951, together with any ancillary information of structural interest. These reports are well systematized, and indeed frequently amount to a short review in a particular field. For example, five pages are devoted to the ferro-electric $BaTiO_3$, on which thirteen papers appeared in eight different journals in 1951. Six pages are devoted to uranium compounds, ten to polysaccharides, and ten to various polymers. There is an excellent coverage of papers from Russian and other journals not easily accessible in Britain, and although, in general, investigations of texture, powder data, and electron diffraction data are not included unless of special structural interest, an exception is made in the case of information from such journals. In general the aim is to present the reader with all the structural details available (sometimes recalculated) without the need to refer to the original paper unless interest lies in the method of analysis rather than in the results of the investigation. These volumes form a series indispensable to scientists in many disciplines.

KATHLEEN LONSDALE

INTERNATIONAL GEOPHYSICAL YEAR

Annals of the International Geophysical Year. *Vol. III, pt. 1, pp. xix + 167; pts. 2–4, pp. 173–381; Vol. IV, pts. 1–3, pp. 201; pts. 4–7, pp. 207–393; Vol. V, pts. 1–3, pp. 244. Pergamon Press Ltd, London.* 1957. *Subscription per volume* £6 *net.*

The primary object of the I.G.Y. is to obtain simultaneous and comparable observations of geophysical quantities, particularly of those connected with the atmosphere. A world-wide co-operative enterprise of this kind presents a very serious problem of communication and exposition. As the participating countries are at very different levels of scientific development and speak many different languages, it is clearly a matter of some difficulty to ensure that the measurements are made and reduced correctly and that the results are presented in an intelligible, useful, and comparable form. The 'Annals of the International Geophysical Year' attempts

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to meet some aspects of this problem. In his foreword to the first volume to be published, Professor Chapman, the President of C.S.A.G.I., the organizing committee of the I.G.Y., says that the Annals will 'indicate the inception, the planning, the preparations and the programmes, and in due course the progress and some of the main results of the I.G.Y.'

Volume III is an instruction manual for ionospheric observations. Part 1 is a 167-page treatise on ionospheric vertical soundings. It is particularly notable for the large number of reproductions of records which will clearly be of great value to those operating stations in remote places. Part 2 deals with the measurement of ionospheric absorption, part 3 with ionospheric drifts, and part 4 with radio noise, whistlers, meteors, and ionospheric scatter.

Volume IV part 1 gives very brief instructions for the measurement of natural and artificial radioactivity in the atmosphere and sea. Part 2 describes the methods of observing the aurora. Part 3 gives instructions for the determination of longitude and latitude. Parts 4 and 5 describe methods of making and reducing measurements of the earth's magnetic field. Part 6 describes the form in which seismological results should be presented, and Part 7 describes instruments for measuring neutron and meson flux.

Volume V part 1 is a substantial handbook on the measurement of atmospheric ozone. Part 2 describes auroral measurements with a camera covering the whole sky. Part 3 is a list of Antarctic radio stations.

Inevitably such a publication as the Annals contains very diverse material, and it is likely to be difficult for the reader to find his way to what he wants. This difficulty is greatly accentuated by the absence of either subject or author indexes. Each part contains a list of contents, but there seems no way, other than turning over the pages and finding these lists, to discover what subjects are treated in a given volume or how many parts constitute a volume. The usefulness of this very elaborate and expensive publication will be seriously restricted if a long series of volumes is produced in which it is next to impossible to find what one wants. E. C. BULLARD

EVOLUTION

A Hundred Years of Evolution, *by G. S. Carter. Pp. x + 206. Sidgwick and Jackson, London.* 1957. 21*s. net.*

Dr Carter has taken the opportunity presented by the centenary of the publication of the 'Origin of Species' to provide in a small compass an attractively written semi-popular history of Darwinism. To say that it is semi-popular is to indicate that there is little in it that is unfamiliar to the professional biologist, though there are few publications in which the material is so compactly and intelligently presented. At the same time, its greatest appeal should be to the scientist in other fields of knowledge, to the intelligent layman, and to schools and schoolmasters.

Dr Carter wisely does not limit himself to the century implied in his title; the early chapters provide the framework of biological knowledge within which the 'Origin of Species' was conceived, and there is a good account of the philosophical, social, and religious climate into which this startling conception erupted. The actual controversies of the late nineteenth century are lightly sketched in, so that in the final chapters the genuine scientific difficulties encountered by the theory in the twentieth century can be explored, and a clear picture of the neo-Darwinian position emerges. A useful glossary and a brief but effective index enhance the value of the book.

J. E. HARRIS

PROTEIN CHEMISTRY

Introduction to Protein Chemistry, *by S. W. Fox and J. F. Foster. Pp. viii + 459. John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1957. 76*s. net.*

A good deal of interesting material is presented here in such a form that it should be easy for students to assimilate it. Since a substantial list of selected references, both to reviews and to original articles, is included at the end of each chapter, the book is also suitable as a basis for further reading. In logical sequence it deals first with amino acids, which take up nearly a quarter of the total number of pages, and then with peptides; there follows a treatment of the properties of proteins in general, and a considerable number of individual proteins is also described. The authors have evidently tried to make the book readable and avoid complexity. The sections which, with a minimum of mathematics, describe physical methods for investigating proteins will be particularly useful to many whose scientific backgrounds are mainly biological or medical. The book contains a generous number of figures, some of which admirably illustrate special techniques. Others, however, are superfluous; how much of significance will the student learn from figure 18-4, which consists of photographs of bottles labelled with the names of human blood plasma fractions? Figure 8-1, which depicts a paper electrophoresis apparatus, is referred to as illustrating paper chromatography.

A personal element must always enter into the selection and weighting of subject matter for a work of this kind. To the reviewer it seems a pity that the conversion of pepsinogen to pepsin was chosen for detailed treatment as an example of the activation of a zymogen rather than the better understood and in some ways simpler case of trypsinogen or chymotrypsinogen. While five pages are devoted to haemoglobin, mention of other haem proteins is confined to a listing of some physical constants of cytochrome C. Phosphoproteins and glycoproteins are also mentioned only in passing—though it must be said in fairness that other more voluminous and comprehensive treatises do little more.

In chapter 9, dealing with the determination of the structure of peptides, no clear distinction is drawn between methods commonly used in recent work and those which are now of largely historical interest. Although a number of the latter type are included, the hydrazinolysis method for C-terminal residues is not. The surprising suggestion is made on page 148 that melting points and mixed melting points are of general applicability for the identification of peptides. The statement, on page 309, that denatured proteins give an α-keratin type of X-ray diffraction pattern is incorrect. On page 372 it is stated that the A and B chains of insulin were separated on columns; no well-known method for effecting this separation in fact employs columns. Perhaps it is unreasonable to expect a biochemical textbook today to be free of inaccuracies. There is a good deal to be said for the view that a major educational function of a young and rapidly expanding subject such as biochemistry is to dispel once and for all from the mind of the elementary student the notion of the infallibility of the printed word. F. R. JEVONS

BOTANY

Il Nuovo Pokorny, *by A. Ceruti. Pp. viii + 680. Loescher, Turin.* 1957. *Lire 4800 net.*

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Pokorny's 'Illustrated Botany', first published in 1836, ran, with revisions, to some 30 editions and was a standard work for many years. The present volume is more than a simple revision: an almost completely new text has been prepared, although the original didactic method of presentation and a number of illustrations have been retained.

The aim of the book is to give a systematic account of the plant kingdom, using as far as possible common 'types' or those most typical of each group. The groups from *Thallophyta* to *Spermatophyta* are arranged according to Engler's system, and the nomenclature is brought up to date in accordance with continental usage. Each group is preceded by a short analytical key using characters intended to draw attention to its fundamental morphology. These keys are simple, practical, and with the exception of the lower groups do not require apparatus more elaborate than a hand lens. For each group a short account is given, under the heading biology, of the life history, some physiology, but little or no ecology. These accounts, although brief and oversimplified, are generally in agreement with modern knowledge. The illustrations are numerous and mostly of a high standard, particularly those specially prepared for this work by U. Tosco, though some reproductions from other authors are not of the same quality.

The author claims that this work should enable students and the intelligent amateur to obtain a wide knowledge of botany by working on their own. Although particularly suited to the teaching methods of the continental gymnasium it is probably too superficial on some parts of the subject to be of the same usefulness to British schools.

F. H. WHITEHEAD

BIRD DISEASES

Tratado de Doenças das Aves (*second edition*), *by J. Reis and P. Nobrega* (*with the collaboration of A. S. Reis, R. C. Bueno, and M. Giovannoni*). *Vol. I, pp.* 391; *vol. II, pp.* 416; *vol. III, pp.* 318; *vol. IV, pp.* 428. *Instituto Biológico de São Paulo, São Paulo.* 1956. *Cruzeiros* 950 *net.*

When this book first appeared in 1936 it was apparent that it would be the standard work for years to come. The present, long-delayed, but none the less welcome edition amply confirms this. The literature covering all aspects of avian disease has been comprehensively reviewed up to 1954, with the inevitable expansion of the book to almost four times its original size. It is now divided into four volumes, which have been paired to form two books. Most sections have been largely rewritten, and a considerable number of new illustrations have been added. The half-tones among these have suffered somewhat by being printed on a cheaper paper than that used in the first edition, but on the whole they are still good. There are bibliographies at the end of each chapter, the value of which alone make the book a necessity for the avian or comparative pathologist. For example, the chapter on Newcastle disease has 434 references and that on Pullorum disease 548, and this extensive review of the literature is maintained throughout.

Volume I deals with virus diseases and covers the field very thoroughly, even down to a virus infection of owls. The section on leucosis, which is inexplicably separated from the rest of neoplastic disease, has a good introductory section on avian haematology. There is a well-balanced review of the controversial issue regarding the relationship of the leucoses with fowl paralysis, and with the filterable tumours. The photograph of the ovarian endothelioma (figure 136) is not convincing, and there is no histology to support the diagnosis of these very debatable tumours.

An omission of some importance occurs in the section (17) which deals with diseases of uncertain aetiology. Round heart disease, although admittedly of limited geographical distribution, has a relatively abundant literature, and should surely have been included.

Volume II describes diseases due to bacteria and moulds and is excellent throughout, although the usage of *Actinomyces necrophorus* for *Fusiformis necrophorus* is a little confusing at first sight for some pathologists.

Volume III, diseases due to protozoa and arthropod parasites, contains a very welcome review of toxoplasmosis in birds, which is of considerable interest, and relevant to the increasing recognition of the widespread pathogenicity of this rather mysterious organism. The section on arthropod parasites is illustrated by excellent line drawings.

Volume IV covers the remainder of the field, i.e. helminth diseases and nutritional diseases, which are dealt with in detail, followed by briefer accounts of systemic and special organ diseases, vicious habits, poisons, developmental abnormalities, hygiene, general therapeutics, and surgery. There are eight colour plates at the end of this volume. Only a few misprints not included in the brief errata were detected, and these were of minor importance. All told, this is an authoritative and monumental work which must some day be translated for the wider benefit of the English-speaking world. It is to be hoped that there will not be such a lengthy interval before the next edition.

J. G. CAMPBELL

BLOOD

Le sang et la transfusion sanguine, *by M. Bessis. Pp. viii* + 139. *Dunod, Paris.* 1957. *Fcs* 740 *net.*

This is the first book of a series *La nature et l'homme* directed by the editor in chief of *La Nature*. *Le sang et la transfusion sanguine* is full of wonders, beginning with the recognition by Leeuwenhoek that the 'fat globules' seen by Malpighi in the blood vessels were '*globules rouges*' (red cells), and that they were responsible for the colour of the blood.

We rapidly move forward in time, and the white cells and platelets are discovered. The principles of the phase-contrast microscope and the electron microscope are then explained in a simple way and we are introduced to the fresh flood of wonders they released. The normal process that ends in the launching of red cells into the blood stream is described (it is estimated that 250 thousand million are launched a day) together with abnormalities of the process—and so for white cells and for platelets.

Chapters deal with the coagulation of the blood, the blood groups, blood transfusion, and the fascinating story of haemolytic disease of the newborn in man and in the mules of Poitou.

The book is splendidly illustrated with electron and phase-contrast microphotographs. It cannot fail to interest both the specialist and the general reader who is curious to know about the marvellous fluid.

R. R. RACE

AUTONOMIC NERVOUS SYSTEM

British Medical Bulletin, *Vol. XIII, No.* 3. Autonomic Nervous System, *edited by W. S. Feldberg. Pp.* 153–230. *The British Council, London.* 1957. 20*s. net.*

There would be general agreement that Britain held for many years the leadership in research on the physiology

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and pharmacology of the autonomic nervous system, and the fine assemblage of contributions in this number bears witness to the continuing vigour of British research in these fields. The reviewer has found all the contributions a pleasure to read, and most contained original and stimulating ideas. There were a very few statements which appeared questionable. However, it is impossible to enter into detailed criticism in this brief appraisal. Appropriately the symposium opens with two very good accounts of the metabolic processes concerned in the production of the autonomic transmitter substances —surely the best in the literature. There are also several good accounts of the various actions of chemical transmitters released by the cholinergic and adrenergic postganglionic nerve terminals. The symposium is further broadened by including critical contributions on various exceptional types of autonomic action and on anatomical and regenerative findings of particular interest to the clinical neurologist. Thus the scope of the symposium is so extensive that some aspects of it will be of direct interest to almost all medical scientists and comparative physiologists. One cannot bring this account to a close more appropriately than to quote from the final sentence of Dale's introduction, that this symposium 'gives the reader an exhilarating sense of the imminence of further developments, needing only special skill, allied with scientific vision and bold enterprise, to open new and ever widening vistas of knowledge, concerning junctional transmission in the nervous system as a whole'. J. C. ECCLES

DANGEROUS INDUSTRIAL MATERIALS

Dangerous Properties of Industrial Materials, *by N. Irving Sax. Pp. v + 1467. Reinhold Publishing Corporation, New York; Chapman and Hall Ltd, London.* 1957. £9 *net.*

This book is a necessary revision of the author's earlier work, 'Handbook of Dangerous Materials'. It has been increased in size and now deals with more than 8500 materials. In addition, the presentation of the data has been rationalized. An attempt has been made to organize the vast mass of information by correlating the properties of the materials listed in Section 10 (general chemicals) with nine previous sections, each dealing with an aspect of prevention and control of hazards which may arise in the use of these materials.

These nine sections are the outstanding feature of the book. Because of the extent of the field covered the information in them is presented in a very concise form, and is mainly confined to elementary principles. These principles are sound and clearly stated. However, there are a number of instances of overlapping of subject matter from one section to another. For this reason, and because of the amount of information in them, these sections would be of much greater value if they were indexed.

The data included and their presentation are such that the book is a useful, immediate reference work dealing with the safety of all who are involved in the use of hazardous materials. That is its objective. W. M. DIGGLE

SCIENCE OF COSMETICS

Cosmetics—Science and Technology, *edited by Edward Sagarin. Pp. xix + 1433. Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1957. $25 *net.*

This work is correctly described by its editors as 'the first collective volume in the history of cosmetic literature'. Its contribution to the science of cosmetics, rather than to its art, is considerable and has been accomplished by the laborious task of bringing together fifty-three sections, each with full references, and contributed by specialists in the restricted fields concerned. Each class of product is covered, from hormones through toothpastes to depilatories, both in purpose and examples of formulation, while other sections give excellent appreciations of manufacturing and physiological aspects. Modern proprietary raw materials are well covered and are identified, together with the manufacturer, by index: British readers will regret the necessity of limiting these materials largely to those of American origin.

As a general reference volume the work is a *sine qua non* to those actively engaged in the industry and provides an essential addition to the classics of de Navarre and Harry. Without detracting from its value, however, some criticism must be made. Firstly, the space given to some aspects seems misjudged in relation to their practical importance, e.g. one page on epidermal water content and permeability rates with no mention of Matoltsy's work seems hardly in correct proportion to six pages on finger-nail elongators. Secondly, a bias by contributors towards personal theories and work has not, in all cases, led to a balanced appreciation; thus the so-called HLB emulsifier selection system is undoubtedly of value, but is not the work of Schulmann also? Thirdly, the work is not free from printing errors. These are, in most cases, only annoying, but in one instance a paragraph has been rendered incomprehensible. At the price, one hopes for perfection.

R. F. L. THOMAS

EARLY HISTORY OF VOLUMETRIC ANALYSIS

The Development of Titrimetric Analysis till 1806, *by E. Rancke Madsen. Pp. 239. G.E.C. Gad, Copenhagen.* 1958. *D. Kr.* 20 *net.*

Dr Rancke Madsen writes of the early history of titrimetry, which he defines as the determination of a substance by its ability to participate in a chemical process. He begins with the work of Boyle on the effect of acids and alkalis on vegetable colours, which, he maintains, adumbrates the titrimetric principle, and he ends with the work of Descroizilles, who is regarded by many as the father of volumetric analysis and who invented the first practical burette. In spite of de Koninck's comprehensive survey, published in French in 1901, it is a pity that the author chose not to extend his period to 1853, when modern volumetric analysis really began with the appearance of Mohr's burette; many interesting, and some important, papers were published in the interval.

In a short review it is impossible to do justice to the author's detailed investigation of the growth of the titrimetric idea and of the changing chemical atmosphere in which it evolved, but tribute must be paid to his careful examination of more than 300 books and papers.

The book, which is a translation, would be easier to read if it had been submitted to an English editor and if the lengthy quotations in antiquated French and German had been rendered into English. Furthermore, the author's attempt to rationalize the nomenclature of analytical chemistry leads him to coin terms that will not be to everybody's taste.

Nevertheless, students of early analytical methods will be indebted to Dr Rancke Madsen for making his knowledge available. C. R. N. STROUTS

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Short notices of books

(These notices are descriptive rather than critical and are designed to give a general indication of the nature and scope of the books.)

Yeasts, *edited by W. Roman. Pp.* 246. *Dr W. Junk, The Hague.* 1957. *Fl.* 25 *net.*

This is Volume 1 of a series 'Biologia et Industria' designed to link the industries using biological raw materials with the work of biologists, chemists, and physicists. It covers the applications of the yeasts to baking, brewing, and the manufacture of wine and saké. The saké and similar yeasts have not been previously described in a European language, and the book also gives the first published account of the industrial production of food yeast written by its inventor. There are a large number of references to the original literature.

The Year Book of the Royal Society of London. *Pp.* 367. *The Royal Society, London.* 1958. 21*s. net.*

This book contains a list of Fellows and Foreign Members of the Royal Society with their current appointments and addresses, the membership of Royal Society Committees and committees formed jointly with other bodies, and the membership of the National Committees for various sciences of the International Council of Scientific Unions. It also includes a list of Royal Society research appointments, and the Statutes, Annual Report of Council, and the Funds and Accounts. Reports of the work undertaken by Professors, Fellows, and Students are also given.

Biographical Memoirs of Fellows of the Royal Society. *Vol.* 3. *Pp.* 328. *The Royal Society, London.* 1957. 30*s. net.*

There are accounts of the lives and work of 21 lately deceased Fellows in this volume, which is the third of a new series, in continuation of the Obituary Notices of Fellows of the Royal Society. The biographies are each prefaced with a photograph and contain bibliographies where relevant.

Handbook of Chemistry and Physics, (39th edition), *edited by C. D. Hodgman, R. C. Weast, and S. M. Selby. Pp. xxii* + 3213. *Chemical Rubber Publishing Co., Cleveland, Ohio.* 1957. £5 *net.*

This is a ready-reference book of chemical and physical data and is intended for the use of the practical scientist. There are many tables of mathematical functions and of a very wide variety of physical constants of substances, including solubilities, heats of formation, X-ray crystallographic data, emission spectra, densities, half-wave potentials (for polarographic analysis), and electron work functions, among very many others.

Physical Chemistry (*third edition*), *by W. J. Moore. Pp. xii* + 641. *Longmans, Green & Co. Ltd, London.* 1957. 30*s. net.*

This is an advanced textbook, written for the American student, but aimed in Britain at those reading for an honours degree in chemistry. It presupposes a knowledge of calculus, elementary physics, and first-year chemistry, and is intended to be used as the sole textbook of theoretical physical chemistry. The subjects covered include kinetic theory, nuclear chemistry and physics, structure of molecules, chemical statistics and photochemistry and radiation chemistry, as well as the normal elementary contents of physical chemistry textbooks, and the book aims at a decidedly advanced treatment. Each chapter ends with problems, answers to the problems (the main difference from the second edition), and a bibliography for further reading.

Solvent Extraction in Analytical Chemistry, *by G. H. Morrison and H. Freiser. Pp. xi* + 269. *John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1957. 54*s. net.*

Solvent extraction methods are methods devised in order to give faster analyses, applicable to trace concentrations and practicable for highly complex mixtures, and this book covers both the theory and practice of the technique. The section concerned with practice deals with apparatus and techniques, a logically arranged survey of the many individual extractions and a selection of procedures for the extraction of elements. There are many references to the original literature.

Laboratory Manual of Batch Distillation, *by F. J. Zuiderweg. Pp. viii* + 126. *Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1957. $3.50 *net.*

The main aim of this book, which is a revision of one by Ir. J. Verheus, is to give help and guidance to the chemist who uses laboratory distillation but does not intend to specialize in the field. The theoretical principles directly bearing on the practice are explained, and the design, construction and testing of equipment are discussed. The more refined aspects, such as continuous distillation and azeotropic distillation, are omitted, but the book contains a survey of the literature which includes references to such matters. Nomographs and diagrams are given in an appendix as well as others in the text.

Le Sostanze Minerali delle Piante in rapporto all'Agricoltura e alla Zootecnia, *by Francesco Scurti. Pp. vii* + 281. *Loescher, Turin.* 1957. *Lire* 4000 *net.*

The chemical elements occurring in plants are subdivided into the macro-elements such as nitrogen, phosphorus, potassium, etc., which are present in large quantities, and the microelements, boron, copper, zinc, etc., of which only minute amounts are found, separate chapters being devoted to each element. Their importance in plant physiology and soil behaviour are considered and, where appropriate, their excess or deficiency is discussed in relation to plant diseases. Each chapter is provided with a full bibliography drawn from international sources.

General Pathology (*second edition*), *edited by Sir Howard Florey. Pp. xv* + 932. *Lloyd-Luke (Medical Books) Ltd, London.* 1958. 84*s. net.*

This book is based on lectures delivered at Oxford to students who have read the Honours School of Physiology. It does not attempt to survey general pathology completely, though some chapters have been added in this edition towards this end, but to deal with subjects in which one or other of the authors has a special interest.

There are 43 chapters treating some of the fundamental changes which take place in the body in response to injury, using this word in a broad sense, and the book is intended to arouse the better student's interest in the functional as well as the morphological aspects of pathology.

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Notes on contributors

J. E. ALLEN,
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Was born in 1928 and educated at the Alsop High School, Liverpool, and at the University of Liverpool. From 1949 to 1952 he studied the pinch effect in high-current spark channels, under the direction of J. D. Craggs, in the Electrical Engineering Department, University of Liverpool. Since 1952 he has worked in the Controlled Thermonuclear Reaction Group at the Atomic Energy Research Establishment, Harwell, and has carried out experimental and theoretical work on various aspects of plasma physics.

HERBERT DINGLE,
D.Sc., A.R.C.S.,

Was born at Brixton, London, in 1890, but spent his early life at Plymouth, where, after leaving school, he was for some years employed by the local education authority. In 1915 he entered the Royal College of Science, London, as a student of physics, and after graduating remained at the college as Lecturer, Assistant Professor, and Professor of Natural Philosophy. In 1946 he was appointed to the Chair of History and Philosophy of Science at University College, London, from which he retired as Emeritus Professor in 1955. He is a member of many British and foreign learned societies, and has been secretary and president of the Royal Astronomical Society. In 1936 he was Lowell Lecturer at Boston, U.S.A., and in 1927 and 1932 was a member of the British Government Eclipse Expeditions (the 1940 expedition, of which he was also a member, was cancelled). His scientific work has been chiefly concerned with spectroscopy (especially in its applications to astronomy and medical and industrial problems), relativity, and the philosophical aspects of science.

G. E. R. DEACON,
C.B.E., D.Sc., F.R.S.E., F.R.S.,

Was born in 1906 and was educated at King's College, London. He worked with the Discovery Investigations from 1927 to 1939, making four voyages to the Antarctic and publishing a number of papers on the oceanography of the Southern Ocean. After four years war service in anti-submarine research, he directed the oceanographic research group in the Admiralty Research Laboratory, Teddington, and has been Director of the National Institute of Oceanography since it was founded in 1949.

SIR HAROLD SPENCER JONES,
K.B.E., M.A., Sc.D., D.Phil., Ll.D., F.R.S., F.R.S.E.,

Held the position of Astronomer Royal from 1933 to 1955. He was born in London in 1890 and was educated at Latymer Upper School, Hammersmith, and Jesus College, Cambridge. From 1923 to 1933 he held the post of His Majesty's Astronomer at the Cape of Good Hope. He was knighted in 1943. He has received the Royal Medal of the Royal Society, the Gold Medal of the Royal Astronomical Society, the Janssen Medal of the Astronomical Society of France, the Bruce Gold Medal of the Astronomical Society of the Pacific, and the Gold Medal of the British Horological Institute. He is an Honorary Fellow of Jesus College, Cambridge, a past president of the Royal Astronomical Society, of the British Astronomical Association, of the Institute of Navigation, of the International Astronomical Union, and of the Society for Visiting Scientists. He is president of the British Horological Institute. He is a foreign member of many national academies and an honorary member of many Dominion and foreign astronomical societies.

GEORGE SALT,
Sc.D., F.R.S.,

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The language of science

The progress of science depends upon many factors, and not least among these is the existence of satisfactory arrangements for the rapid exchange of new ideas and experimental results between research workers having similar interests. In a branch of learning that prides itself particularly upon its powers of systematization it is surprising that in this respect so much has been left to chance and so little has been done to formulate and follow an agreed policy appropriate to modern needs. There is, of course, no lack of scientific literature, both books and journals: the 'World List of Scientific Periodicals' now lists some 50 000 titles. Indeed, almost every scientist now finds it impossible to read all the work relevant to his own subject, far less to read extensively outside it. Yet wide reading is becoming increasingly important, for it is very frequently in the fields where several branches of science overlap that the most exciting results are obtained.

While this problem of the sheer quantity of literature is a serious one, it is at least one to which some effective answers have been found and applied. Most important, of course, are the various abstracting services, the value of whose work needs no emphasis here. Moreover, the large number of scientific periodicals is not wholly a bad thing: the less important ones serve to some extent as a filter, though by no means a completely efficient one, that reserves work of proved importance for journals of wider circulation and higher standing.

The no less important question of the language in which scientific work should be presented remains, however, largely unresolved. It is, like so many others in the modern world, one on which it is generally agreed that an international policy is urgently necessary but on which it is in fact difficult to get any agreement at all. Clearly, the problem is one which no amount of good will and harmonious agreement can solve quickly; this is an additional reason for deciding upon the ultimate goal and trying to agree upon a policy by which it may be achieved. In the meantime the barrier of language keeps many scientists in prolonged ignorance of work of immediate importance to them being done in many countries. With the rapid expansion of scientific research in countries whose contribution has hitherto been relatively small the problem is rapidly becoming more serious.

The general facts of the situation are easily ascertained and occasion no controversy. It is established, for example, that much of the world's scientific literature is published in a language unintelligible to more than half the world's scientists. The distribution of journals according to language shows a wide variety. Roughly speaking, some 44 per cent of scientific journals are published in English, 14 per cent in German, 13 per cent in French, 8 per cent in Russian, 5 per cent in Spanish, 4 per cent in Italian, and the remainder in the other languages of the world. Such figures cannot be exact, and they vary from one branch of science and technology to another, but they certainly indicate the general situation. Moreover, these proportions can be expected to change fairly rapidly. Unless some major change of policy occurs, the proportion of papers appearing in the Russian language is bound to rise rapidly. There is every reason to expect that the scientific literature of China will grow rapidly, and it is likely that, if only for political reasons, much of this will be published in Chinese, with Russian the second choice. It is possible that Hindi and other languages at present unfamiliar to the Western world may in time become important scientific languages. In the absence of positive action the situation is bound to become steadily more complicated and difficult to resolve.

Supposing—and in present circumstances it is a supposition requiring a good deal of optimism—that international agreement could be obtained to mitigate these difficulties, what is the proper policy to pursue? Three principal courses are clearly open to us, presuming that the present one of laissez-faire is rejected. Firstly, we might aim at the ideal of a single language for the communication of all scientific work. Secondly, a limited number of languages, selected from those already most widely used for scientific purposes, might be selected. Thirdly, more extensive use could be made of professionally prepared translations.

In considering these general proposals it is necessary to consider both short-term and long-term policy. To satisfy immediate needs, taking the situation as it is and not as one might wish it to be, the extension of translations between languages is the only possible policy. This has, of course, been recognized by ENDEAVOUR since its inception in 1942: at the present time we look upon our foreign-language editions as indispensable to our object of providing an internationally

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useful review of the progress of science. The recent great expansion of professional interlingual scientific translation is evidence that this view is generally accepted. Much of this expansion has, of course, been occasioned by the relatively sudden efflorescence of science in Russian, which has introduced a novel factor into the situation. Here we have a sudden and unprecedented flow of important scientific work in a language with which the scientific world in general is not merely unfamiliar but totally ignorant. In the circumstances no choice is open to the West: for the moment the work must be followed through translations or not at all. It should be remarked that this situation apparently does not exist in reverse. At least fifty per cent of Russian scientists and technologists are reputed to be able to read English, French, or German, and many are said to know more than one of these languages.

Indispensable though it is, there are obvious reasons against indefinite extensions of the translation system. It is wasteful of time, money, and scientific manpower; it causes delay; and it can lead to misunderstanding. So far as the last point is concerned, it is perhaps not generally realized how difficult it is to make really accurate translations of highly technical material. In fields developing rapidly, essential words may not even have been coined in certain languages, and only knowledgeable circumlocution can render the sense accurately. While an increase in translation services seems inevitable over the next few years, it is a process not without danger. It will lead to the evolution of an extensive and expensive organization whose very existence will be prejudicial to the adoption of alternative policies that may in the long run be more satisfactory.

A universal language for all scientific communication is obviously desirable on many grounds, but both its choice and its achievement are likely to be beset by great difficulties. The choice of any single language for the communication of results in so important a field would naturally be beset by all kinds of political and nationalistic obstacles. Many of these might be overcome if one adopted a non-living language such as Latin or an artificial language such as Esperanto or Interlingua. Artificial languages are open to the fundamental objection that their very nature and purpose make them unsuitable for conveying subtle shades of meaning: they are satisfactory enough for describing facts but inadequate for conveying ideas, especially unfamiliar ones. Latin is free from this defect, for in its long evolution it acquired considerable flexibility. Its evolution virtually stopped, however, before modern science began. Its present vocabulary and syntax are therefore inappropriate for modern needs, as is well known to every university orator who has been called upon to describe in Latin the work of scientists. It is a great pity that Latin was allowed to lapse as the international language of scholars, but it is doubtful whether it is now feasible to revive it.

The choice of a single language would have the great and obvious advantage that nobody would be required to learn more than one language other than his own. The argument in its favour is further strengthened by the consideration that for the innovation to be effective it would be necessary for all scientists to be able not merely to read this language—a facility that could be acquired without great difficulty—but to write in it, which is very much more difficult. To learn to write well in even one foreign language is for the ordinary person a considerable and time-consuming achievement.

A compromise, by which two or three modern languages only are accepted for universal use in science, seems more likely to achieve success, but even if a decision were taken today it could have effect only after many years. For this to be effective, it would be necessary to institute educational reforms in many countries, for real proficiency in a foreign language is not easily acquired without formal study. These, even if achieved, could not bear fruit for a long time. The certainty of difficulty is, however, no argument for making no attempt to reach a generally acceptable policy. At least the teaching of languages is a feature of most national curricula, and it ought not ultimately to be impossible, when so much is at stake, to agree upon the substitution of one for another when necessary.

The barrier of language is harmful in science, but it is, of course, an equally serious obstacle in all fields of learning, in politics, in industry and commerce, and in every kind of dealing between nations. It might well be that if scientists were to give a lead, others would follow. While it is manifestly not true that a common language ensures harmony between nations it certainly promotes clearer understanding, and at the present time there is nothing more urgently necessary than this.

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Some factors governing the development of the earth's crust

V. V. BELOUSSOV

The mechanisms governing the movements of the earth's crust are exceedingly complex. Nevertheless sufficient order can now be seen in them to allow some understanding of how the earth's crust has reached its present form; to make some deductions useful to mining geologists and others concerned with immediate practical problems; and even to predict changes likely to occur in future ages. These problems are engaging the attention of geophysicists throughout the world, but this review is particularly concerned with research carried out in the field and with models in the Soviet Union during the past twenty years.

Everyone knows that the earth's crust is not at rest. It is always in motion, rising slowly in some places and sinking in others; sudden movements are expressed in earthquakes. The variety and complexity of crustal movements, and the laws that govern them, however, are apparent only to specialists. Such tectonic movements of the crust reflect the internal life of our globe, but this life is still very inadequately understood. The study of these movements, their variety, sequences, and mutual relations, is one of great scientific interest. Strangely enough, processes taking place in the depths of distant stars are in some ways better known to us than occurrences in the interior of our own planet. At the extremely high temperatures and pressures which prevail in the interiors of stars, matter undergoes such profound alteration that its properties are simplified and become amenable to mathematical idealization. Inside the earth, however, while the conditions are such that we must assume that they modify the properties of matter, they are undoubtedly different at different depths, and the molecular and atomic bonds are not completely disrupted. It is therefore very difficult to establish the properties of matter in the interior of the earth, at least so long as it remains impossible to reproduce the conditions experimentally.

The laws controlling the movements of the earth's crust must be taken into account in speculations concerning the processes taking place inside the earth and in the formulation of theories of mountain-building and of the origin of earthquakes. In dealing with this last topic, a knowledge of tectonics has a practical importance, since it provides a basis for working out methods of forecasting the time and place of earthquakes.

A knowledge of the factors governing tectonic movements acquires a still greater practical importance as a result of the way in which the present structure of the earth's crust has been built up. The distribution of sediments, the extent, composition, and thickness of these sediments, and the disposition of folds and faults appear to represent the sum of the effects of past tectonic movements. Knowledge of the history of these movements enables us to predict the arrangement and shapes of the masses of different rocks. Certain types of mineral resources are associated with particular rock-types or with particular structures such as folds or faults. The principles governing the tectonic development of the crust therefore provide the basis for many means of estimating the distribution of mineral resources, and of constructing metallogenetic charts, maps showing the probable distribution of oil-bearing strata and coal seams, and so on.

The branch of geology concerned with the causes and effects of movements in the earth's crust is known as geotectonics. This science has been extensively pursued in the Soviet Union during the last fifteen or twenty years, in both its theoretical and its practical aspects. Some fundamental contemporary ideas on tectonics are briefly set forth below. This article deals with the tectonic processes which are taking place, or have taken place, in those parts of the earth's crust that make the continents; it does not deal with the structure and development of oceanic basins. The author's account is based mainly on the results of Soviet investigations, and to some extent on his own work.

Study of the earth's crust shows that it undergoes various kinds of movements. Several schemes

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for the classification of these movements have been proposed, but we shall confine ourselves to the simplest of them, classifying movements as oscillatory, fold-forming, and fault-forming. Movements of the first type result in slow elevation or depression of the crust; the second type results in the crumpling of beds into folds, and the third in the formation of structural breaks or faults.

These types of movements are not all of equal significance. The oscillatory movements are of fundamental importance. They occur throughout the earth's crust and serve as a background against which folds and faults are produced in some places during some periods of time. There are grounds for suggesting that oscillatory movements are the basic cause of fold-forming and fault-forming movements.

OSCILLATORY MOVEMENTS

Oscillatory movements which are taking place today can be studied most accurately by geodetic methods, for example by repeated accurate levelling. The effects of movements which took place within historical times can be established by the evidence of towns inundated by the sea as a result of sinking of the land, or of ports set back from the sea by uplift of the land, and so on.

Geology is concerned in the main, however, with the oscillatory movements which took place in former geological periods. These movements left their traces in the crust in the form of sedimentary deposits whose composition and thickness varied from place to place.

The sedimentary strata which are found in the continental areas are deposits laid down either in shallow water or on low-lying land such as deltas and coastal plains. Evidently, deep-water oceanic basins have not in general occupied the site of these continental platforms. Of the deposits formed on dry land, only those accumulating in low-lying regions have survived, those deposited on higher ground (for example, in river valleys) were as a rule rapidly eroded away by the action of water. The accumulation and retention of sediments are in themselves always evidence of the subsidence of the earth's crust. Thus, the parts of the crust where sediments have accumulated must have been regions of subsidence at the time of accumulation.

The composition of deposits depends to a considerable extent on what was going on at the time of accumulation in neighbouring regions which were being elevated. If uplift took place rapidly and with considerable force, these regions were subjected to rapid erosion and large quantities of coarsely broken up material were carried from them to the subsiding regions. If uplift was slow, then erosion was sluggish and only a little sludgy material was carried away, to be deposited on the periphery of the regions of subsidence. The remaining, larger, parts of these regions were filled up with organic deposits of local origin, predominantly limestone. Thus, by determining the distribution and composition of sedimentary strata of various ages it is possible to assess the former distribution of regions of subsidence and uplift, and to estimate the relative rate of uplift.

Further information can be gained by measuring the regional variations in the thickness of deposits of a particular age. For example, the Jurassic deposits in the centre of the Russian lowland have a thickness of the order of tens of metres, whereas those of the Caucasus reach a thickness of some thousands of metres. It has been established that shallow sea and lowland continental deposits accumulate to the extent that the subsidence of the crust allows, and that their thickness corresponds closely with the final amount of subsidence. Thus it is possible to separate out zones of greater and less subsidence within regions of deposition. The example mentioned above shows that during the Jurassic period the crust subsided both in the Russian lowland and in the Caucasus, but the amount, and hence the rate, of subsidence was considerably greater in the second region than in the first.

These and other methods have been used to study the history of oscillatory movements in various regions. In the Soviet Union work of this kind has been carried out on the Russian platform, in the Caucasus, the Crimea, Tien-Shan, Kopet-Dag, the Urals, and many other regions. In the course of this work some of the factors governing the development of oscillatory movements have been determined, and these will now be briefly discussed.

When oscillatory movements are taking place, the earth's crust is always divisible into regions of subsidence and uplift. Some general periodicity can be observed in the sequence of these movements; at some times uplift predominates in all areas and at others subsidence predominates everywhere. This fact enables us to recognize geotectonic cycles in the history of the earth. Each cycle begins with a phase of predominant uplift in which the area of dry land reaches a maximum, the relief is high and the seas are shrunken; further development leads to a phase of

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predominant subsidence in which the dry land shrinks and becomes lowland, while the seas expand. The cycle finishes with a renewal of uplift and a shrinking of the seas. A new cycle then follows, with the same sequence as before.

This 'breathing in and out' of our planet is one of its most remarkable characteristics. The major cycles are of very long duration—a rough estimate is of the order of 150 million years. The best-known cycles are the last three which are called the Caledonian, Hercynian (or Variscan), and Alpine. The first of these covers the Cambrian, Ordovician, and Silurian periods, the second the Devonian, Carboniferous, and Permian and the third the Triassic, Jurassic, Cretaceous, Palaeogene, Neogene, and Quaternary. The periodicity of the oscillatory movements controls the recurrence of strata of similar composition in the stratigraphical succession (figure 1). Since certain mineral resources are associated with particular types of sediment, the same periodicity determines the distribution of these resources. Thus, for example, two large reserves of oil and coal are known in the earth's crust; one of them is associated with the Carboniferous system, the other with the Palaeogene (figure 2). The principal sources of salt, gypsum, etc., are the Silurian, Permian, and Neogene deposits. The last or Alpine cycle exhibits some irregularity in its development. The transitions from one phase of this cycle to the next took place at different times in the Pacific region (East Asia, the west of North America) and in the Atlantic region (Europe and the east of North America).

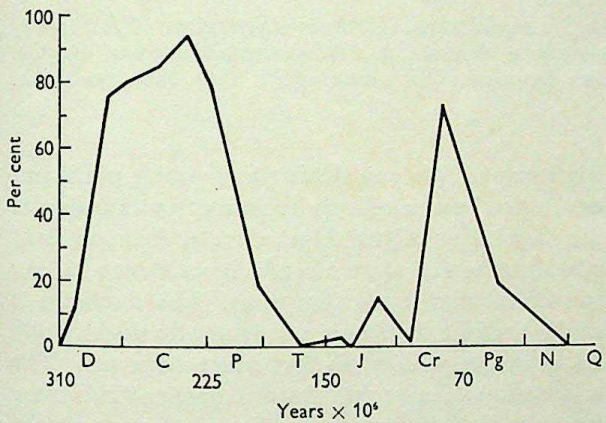

FIGURE 1 – *Percentage change of limestone content in the middle of other rocks on the Russian platform during the Hercynian and Alpine cycles (according to A. B. Ronov).*

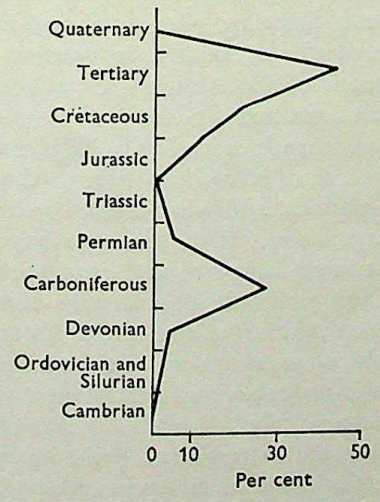

FIGURE 2 – *Distribution of world petroleum resources according to geological systems (according to I. M. Gubkin).*

An interesting feature of the oscillatory movements is their complexity: movements of different orders are always superimposed one on the other. Subsidence is complicated by local or partial uplift, and elevation by partial subsidence. The mutual superposition of movements of different orders, which are obviously governed by processes taking place at various depths in the earth, makes it possible to express, say, a particular uplift either as absolute—that is, as a raising of the earth's surface relative to sea level—or as relative, taking, for example, the form of a subsidence less pronounced than that of surrounding areas. If a partial uplift takes place in a region of general subsidence, the final result will obviously be the algebraic sum of both movements. In periods of general subsidence, uplift will usually be only relative, whereas in periods of general uplift it will be absolute.

During the last three cycles, and probably also during the preceding three or four cycles, the continental masses were divided into regions of two types—geosynclines and platforms. Geosynclines are characterized by extraordinarily intense and opposing oscillatory movements. They always consist of narrow elongated zones in which differential movements of opposite sense and high velocity take place, the velocity reaching a few millimetres or sometimes a few centimetres per year when averaged over a long period. In belts of subsidence occupied by the sea, sedimentary strata of very great thickness (up to 10–12 km) accumulate and uplifted zones take the form of island mountain ridges. At the end of a tectonic cycle, in the phase when uplift predominates over subsidence, high mountains are formed on the site of geosynclines. The disposition of regions of elevation and subsidence does not remain constant throughout a tectonic cycle. Uplifted regions appear within areas of subsidence; the residual

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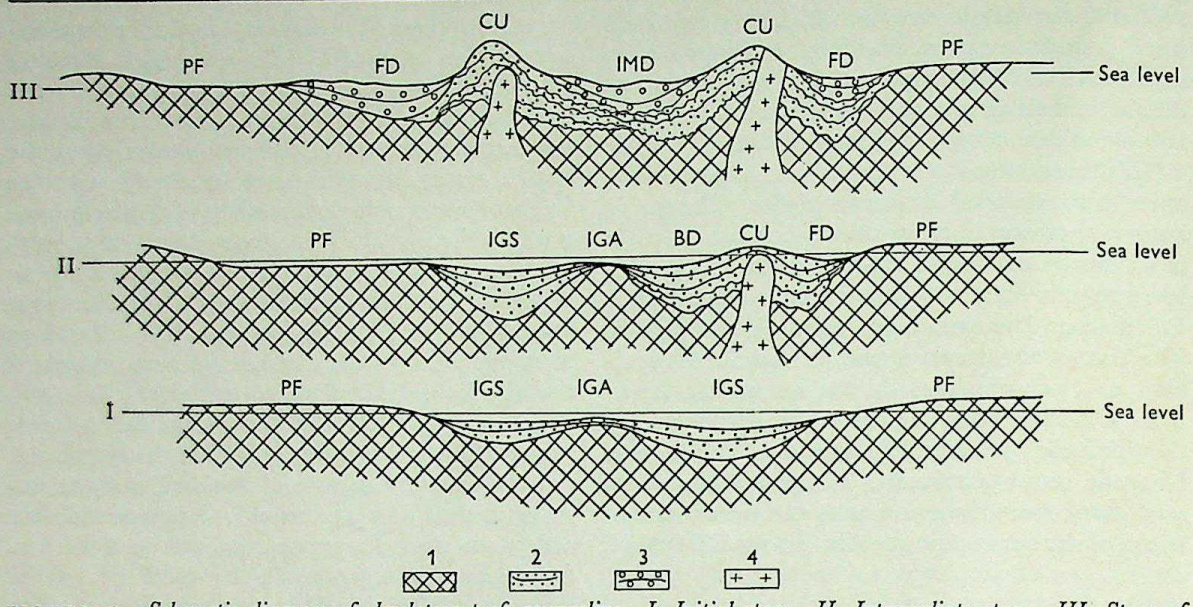

FIGURE 3 – *Schematic diagram of development of geosyncline. I. Initial stage. II. Intermediate stage. III. Stage of mountain formation. 1. Ancient sub-structure. 2. Predominantly marine deposits. 3. Predominantly continental deposits. 4. Granites. PF. Platform. FD. Fore-deep. IMD. Intermountain depression. CU. Central uplift. IGA. Intra-geanticline. IGS. Intrageosyncline. BD. Marginal depression.*

basins are gradually pushed aside like waves and encroach on former zones of uplift so that regions of uplift and subsidence often change places towards the end of a cycle (figure 3). The main Caucasian mountain range, for example, was raised in the Tertiary era from a region where one of the deep basins of the Caucasian geosyncline existed in the Mesozoic. The depression of the river Kura to the south of the main range was formed on the site of a much earlier uplift.

Platforms are characterized by feeble oscillatory movements of low velocity and small amplitude, with smooth transitions from regions of uplift to regions of depression. The distribution of elevated and depressed regions is considerably more stable than in geosynclines and changes less during a cycle, although the platform as a whole moves according to a general rhythm, first sinking and then rising.

In each cycle, the areas occupied by platforms increase at the expense of areas occupied by geosynclines. The earlier cycles are distinguished from the later ones by the more rapid growth of platforms and this has been one of the most important elements in the development of the crust, transforming the tectonic cycles from closed circles, as it were, into loops of a spiral.

Since in each cycle regions previously occupied by geosynclines became converted into platforms, the different parts of a platform may be of different ages. We can distinguish young platforms developed comparatively recently, for example at the beginning of the Alpine cycle, from ancient platforms which were already in existence in, say the Caledonian cycle (figure 4). The movements of the ancient platforms are generally slight while those of the young platforms are stronger. The conversion of a region from a geosynclinal state into a state characteristic of a platform may be such a gradual process that it is expedient to recognize an intermediate stage in which the region can be said to form a parageosyncline. The Russian and Siberian platforms are examples of ancient platforms, while the western part of Europe, north of the Alpine geosyncline, represents a recent platform.

These considerations lead us to inquire what the earth's crust may have been like in the earliest geological periods, and what it may be like in the very distant future. If the changes have always gone in the same direction, and continue to do so, then it seems probable that in the distant past there were no platforms and in the distant future there will be no geosynclines. In the most ancient periods, it is probable that platforms were either small or completely absent, and intense movements took place everywhere. Some investigators, however, consider that in Archean eras the crust was in some special state to which the concept of geosynclines and platforms did not apply. So far

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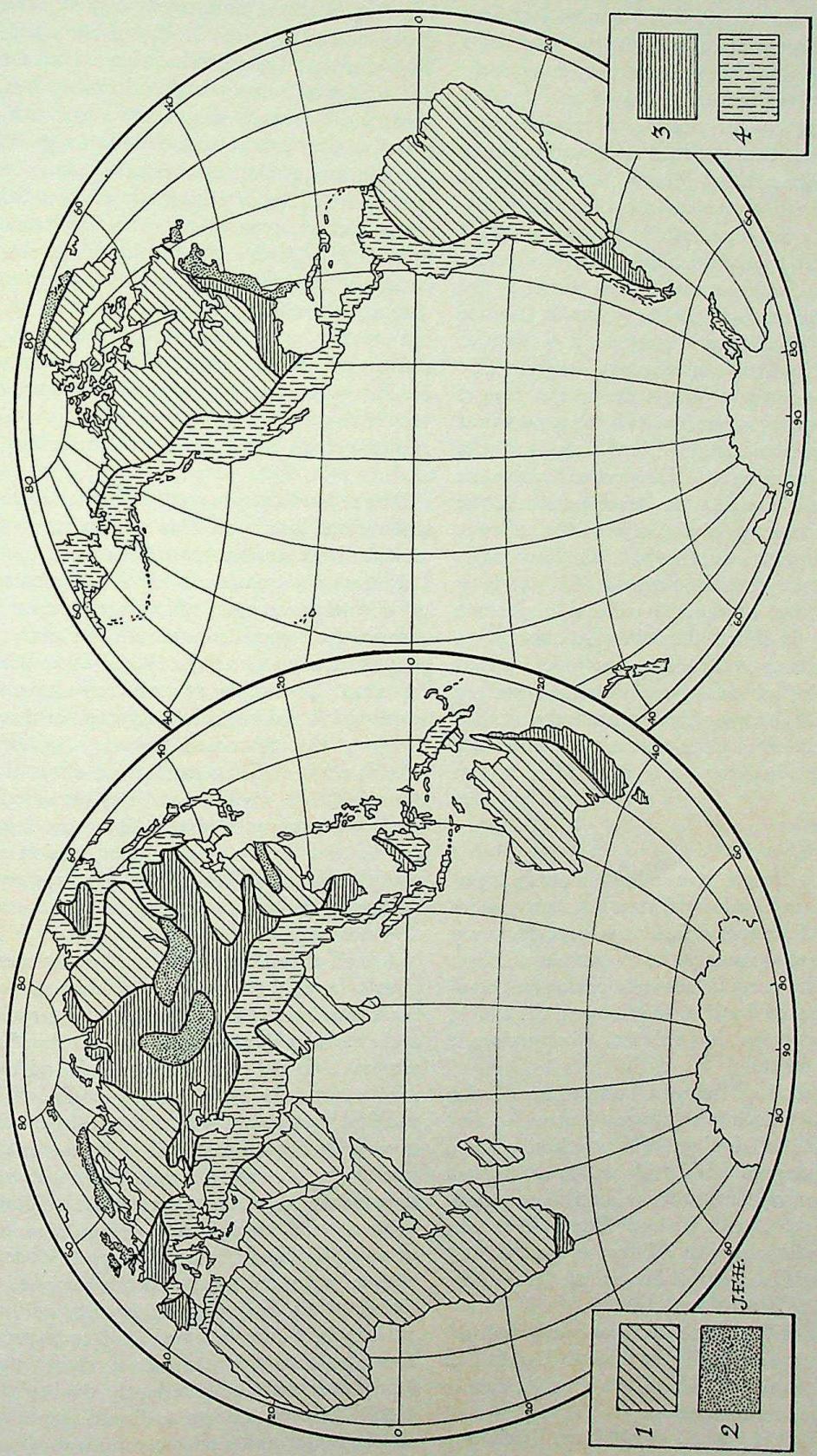

FIGURE 4 – *Tectonic scheme of the earth.* 1. *Ancient platforms.* 2. *Platforms on Caledonian folded base.* 3. *Platforms on Hercynian folded base.* 4. *Existing Alpine geosynclines.*

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as the future is concerned, we have more justification for suggesting that when universal platforms have developed, the crust will enter on an entirely new phase of evolution.

The character of this phase is at present suggested by conditions in central Asia and in parts of Tien-Shan where the high ranges of the Tien-Shan are separated by deep inter-mountain hollows. In these regions, geosynclinal conditions had come to an end by the close of the Palaeozoic eras. Platform conditions existed there during the Mesozoic and most of the Tertiary, but in the late Neogene and Quaternary periods a new and intense series of tectonic movements began. The contrast between the uplifted mountain ranges and the intervening depressed hollows shows that vertical movements attained an amplitude of about 10 km over a period of several million years. We call this new stage in the development of the earth's crust the stage of activation of platforms. It has begun not only in Central Asia but also in southern Siberia (Altai, Sayansk Mountains), Africa (the region of the rift valleys) and some other regions. It shows that the platform is not the final product of tectonic development and that the 'sclerosis' which many authors have written about does not in any way threaten the earth's crust; the platform itself is an unstable structure and intense movements can recur after it has developed.

FOLDING MOVEMENTS

Folding movements, as we have already mentioned, are developed against a background of oscillations and are subordinate to them. Not very long ago, attempts were made to explain all types of folding of the crust in terms of horizontal compression produced by the shrinking of the earth. It is now clear to us that this line of approach is certainly incorrect.

It must be realized first of all that many diverse phenomena of varying significance are classified under the title of folding. The author, with a group of colleagues and students, is at present carrying out extensive studies of a series of regions of folding in the Soviet Union. The field studies are being supplemented by laboratory experiments which are carried out as far as possible under similar physical conditions.

The results of this work are consistent with the view that systems of folding can be divided into at least three types which differ in the mechanism or kinematics of their formation.

First of all there is block folding. This is a bending of strata as a result of vertical uplift or depression of discrete blocks of the earth's crust. The characteristic products are box folds with flat tops and steeply inclined limbs, but experiments with models show that when the block—which acts, as it were, as a die—lies at great depths, the strata near the surface form domes instead of box folds. The development of box folds is in many instances associated with oscillatory movements. The history of a block fold, as revealed by its structure, often indicates that it grew slowly throughout whole geological periods and was not infrequently subjected to alternating upward and downward movements. The connection with oscillatory movements, which is a genetic one, is very close; we can recognize actual oscillatory movements in box folds, but they are on a small scale.

The second category of folding is folding due to differential loading. This results from the horizontal displacement of material in a layer which becomes highly plastic at depth. As a consequence of this displacement, the plastic layer becomes thin in the region from which the material flows and is thickened at the place into which it is injected. The overlying strata sag above the place from which the material migrated and are raised up over the thickened region. Sometimes the injection has such a pronounced effect that 'cores of penetration' are formed. The peculiarity of this kind of structure is that it does not continue in depth. Below the plastic layer, which plays the active role in folding, the rocks may all lie horizontally, or may be deformed in an entirely different way.

A lack of uniformity in the load resting on the plastic layer, and a low specific gravity in this layer relative to that of the overlying rocks are the main causes of horizontal flow in subsurface plastic material. If the depressions produced by vertical movements are filled up by sediment and the uplifted regions are eroded, the load on the plastic layer becomes uneven. Equilibrium is disturbed, and the plastic material may begin to flow from the depressions towards the uplifted regions. A reversed relief, with high ground over tectonic depressions and lower ground over uplifted regions, may contribute to this process. It is also favoured by low specific gravity of the plastic rocks which makes them tend to move upwards from beneath the heavier overlying beds. This last factor acts independently during the formation of what are known as diapiric domes, in which a core of light salts or gypsum slowly floats up like

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a very viscous liquid through heavy sands or clays overlying it. Folding due to differential loading may complicate block folding in regions where there are thick plastic layers in the succession.

The final type of folding is a general crumpling produced in parts of the earth's crust where longitudinal flexures are developed under the influence of horizontal compression.

The causes of horizontal compression should not, as our investigations have shown, be sought in world-wide processes. It is fundamentally much sounder to seek these causes in more local processes, particularly those related to geosynclines where general folding predominates. Opposed oscillatory movements often result in the division of the crust into blocks, and the slopes connecting uplifted and depressed blocks produce a step-like structure. When blocks are raised above their surroundings, the upper parts tend to slip outwards, setting up a pressure on neighbouring lower-lying blocks whose strata are thus crumpled. An isolated elevated block acquires a mushroom-shaped section and exerts a pressure on the blocks on either side. Where blocks follow one another step-wise, every block presses on the one below it. The blocks thus crumple one another in succession, with the exception of the uppermost, whose structure would appear as a simpler one.

Other local causes of general folding can also be related to opposed oscillatory movements: for example, if a certain block first rises and then sinks, the strata covering it will be stretched during the period of uplift and then thrown into folds when the block sinks again. Multiple oscillatory movements produce more numerous and more complex folds. Other examples of factors producing general folding could be mentioned; they all have in common the fact that they are related to opposed vertical movements in the crust. General crumpling therefore occurs predominantly in geosynclines.

The mechanical causes of the outward slipping of the upper parts of raised blocks are twofold. In the first place, there is the simple effect of gravity; in the second, there is the squeezing out of material from within the crest of the rising block as a result of the resistance set up by the overlying strata. Both processes are easily reproduced in models.

Until recently, geologists who were brought up on survivals of the contraction hypothesis did not ask themselves about the causes of formation of folds in restricted areas, since they considered folding to be a general phenomenon capable of being interpreted only on a regional scale. Now, however, we need to establish the local cause of folding; this may vary from place to place, although, as has been shown already, it is generally related to opposed oscillatory movements in geosynclines.

The various types of folding are, as the previous pages suggest, connected in a systematic way with a regime of oscillatory movements. Block folding always occurs in platforms and in geosynclines. It is more intense in the latter, but as it appears to be the only type of folding which occurs in the former, its independent occurrence is, in general, characteristic of platforms. It is usually stronger in young platforms than in ancient platforms.

Folding due to differential loading requires the presence of thick successions containing layers of plastic rock. Favourable conditions exist in deep depressions on platforms where saliferous or gypsiferous layers are not uncommon in sedimentary successions. In these circumstances, folding most frequently produces diapiric domes. Still more favourable conditions exist in the fore-deeps which are located between geosynclines and neighbouring platforms. Here, the successions are even thicker, and may contain thick layers of salt, gypsum, or plastic clay. Not only diapiric domes but also elongated folds produced by injection, separated by wide sloping depressions, may be formed: this is known as coulisse-folding, and has the appearance of a comb.

Finally, the geosynclines themselves, deep depressions filled in by very heterogeneous rocks, provide conditions which are no less favourable for folding of the type discussed above. This type of folding is found in geosynclines, but is dominated by general crumpling and is not always distinguishable. For this reason, block folding in geosynclines does not attract the attention that it deserves.

General crumpling, as has been shown, demands differential vertical movements and hence it is associated as a rule with geosynclines, where it masks the appearance of other types of folding. It also occurs on a small scale in isolated regions and in platforms where differential movements take place locally, for example in deep rift valleys.

There is a definite relation between the place and time of development of general folding on the one hand, and the progress of oscillatory movements on the other. The folding begins on the flanks of blocks rising inside subsidiary depressions and encroaches on an ever-increasing area with the continued elevation of the blocks (figure 3).

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FAULTS

Tectonic faults are formed where stresses in a rock exceed its strength. They develop under conditions of tension, compression, or shearing, for example when beds are arched up and thus tend to increase their area. Any uplifted mass has, to a greater or less extent, a tendency to be cleft by longitudinal, transverse, and radial cracks. The small blocks defined by these cracks are displaced upwards or downwards with the formation of normal faults. Under conditions of local compression, thrusts, strike-slip faults, and so on are formed in association with folds.

The behaviour of faults can be studied by means of models, and is explicable in terms of physical principles. These studies have served Soviet tectonicians as a basis for the development of the discipline known as tectonophysics, which is the application of physical methods and concepts to the investigation of the deformation of the earth's crust. The mechanism of formation of faults has been extensively studied in the Soviet Union; in particular, it has been found that many interesting features of faults arise from the fact that they are produced not by cracking but by viscous flow following plastic deformation.

Deep faults present a special problem which is a long way from being solved. There are grounds for supposing that at least some parts of the earth's crust are fractured by deep faults which penetrate even to the mantle. These faults, beginning as weakened scars, are made use of in the course of oscillatory movements. In addition, it is suggested that they have a systematic direction and determine the course of geosynclines and other tectonic complexes. Their formation is, according to some scientists, connected with changes in the form of the earth due to the slowing down of its rate of rotation.

It is certainly remarkable that, for instance, practically all the active tectonic zones in the region between Lake Baikal and the Crimea have the same north-west strike. These include zones of uplift and subsidence in the east and west Sayansk Mountains, the Altai, the Tien-Shan, central Kazakstan, Kopet-Dag, the Caucasus and the Crimea. Yet the same north-west strike is repeated by the junctions which separate zones of uplift and subsidence in the non-Alpine region of western Europe. Here the zones were clearly formed only at the beginning of the Mesozoic, since at the end of the Palaeozoic era the tectonic zones had a completely different strike.

The role of such deep faults in the structure of the earth and their history present an attractive problem for further study. This touches on the problem of large horizontal movements along deep vertical faults. American geologists are attracted by this topic, as illustrated by the San Andreas fault; Soviet geologists do not share their enthusiasm and in fact have strong doubts as to the reality of large horizontal displacements.

Another interesting problem which we have not touched on is the origin of continents and oceans. If we had turned our attention to this problem we might have drawn on material from geophysical sciences, but this is not within the scope of the present article. Still less can we deal here with the possible causes of tectonic movements, since this involves not only geophysical but geochemical considerations.

What has been said, so it appears to us, is enough to show how complex, and at the same time how systematic, are the mechanisms associated with movements in the earth's crust.

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The biochemistry of the eye related to its optical properties

ANTOINETTE PIRIE

Many of the experimental studies of the physiology of vision are concerned with the light-sensitive pigments of the rod and cone cells of the retina. These are essential for light reception, but so also are other parts of the eye such as cornea and lens. This article considers some aspects of the biochemistry and physiology of the parts of the eye through which light reaches the retina, dealing with the eye both generally and with reference to the specialized adaptation developed in some animals to assist their vision in poor light.

The theme of this article is the relation between the biochemistry of the eye and its function as a light-transmitting, rather than a light-receiving, organ. The rod and cone cells of the retina that contain the light-sensitive pigments and the neurones of the retina and the optic nerve are concerned with light reception and vision, while the rest of the eye—the refractile and transparent parts, such as the cornea, the lens, and the vitreous body—are essential for the transmission and focusing of light on to the light-sensitive cells. These parts of the eye (figure 1) must be both transparent and optically uniform—if the liver or the little finger grows by as much as a millimetre the change is not noticeable, but increase the length of the adult eyeball by a millimetre and abnormal vision is the result. Change in shape of any part of the eye because of a biochemical or pathological lesion can be disastrous for function. Ankles can and do swell in hot weather, but however uncomfortable this may be it is still possible to get about. But if the cornea swells even microscopically, because the respiration of its epithelial cells has been checked through lack of oxygen, vision is distorted and the discomfort of the eye is intolerable.

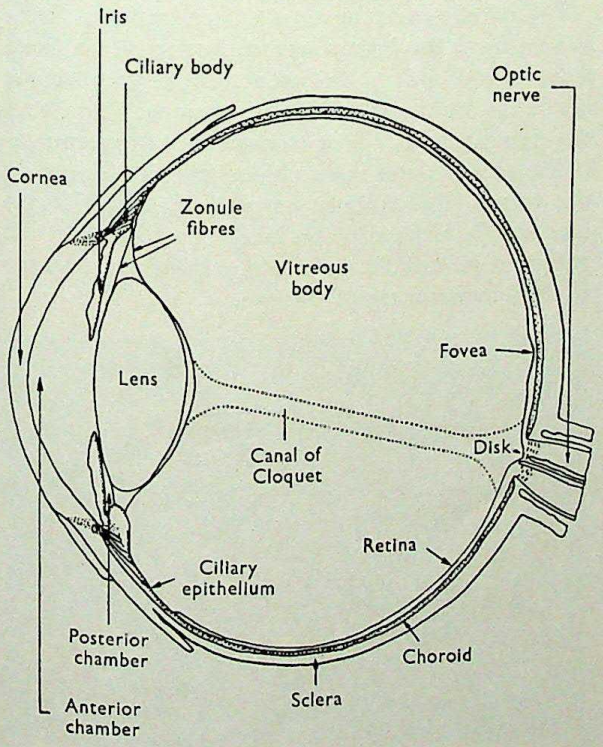

FIGURE 1 – *The human eye in section.*

Quite simple experiments show that the transparency and optical properties of cornea and lens depend on their metabolism. One such series of experiments analysed the visual difficulties produced by contact lenses.

The cornea, which is the transparent circular area of the outer coat of the eyeball, consists of three parts: the epithelium, which is made up of several layers of box-shaped cells; the stroma, which consists of interlacing collagen fibres with a few cells in the interstices (figure 4); and a single layer of endothelial cells on the inner surface. Now people who wear contact lenses may find that after a few hours their eyes start to irritate and they can no longer see clearly. Vision is blurred and distorted, and anything brightly lit appears to have a halo round it. Removal of the contact lenses restores normal vision. Examination of the eye when it is in its irritated state shows that some of the corneal cells are swollen, and it is these swollen cells that cause blurring and distortion by irregular refraction of the light rays.

G. K. Smelser [1–3] has investigated the problem both in guinea-pigs and also in himself. He

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fitted the pigs with contact lenses and later examined the cornea both in the living animal and histologically, and found that wearing the lens caused swelling of epithelium with accumulation of fluid between the collagen fibres of the stroma. The glycogen granules of the epithelium disappeared and the lactic acid content rose, pointing to change from an oxidative to an anaerobic metabolism. So Smelser fitted contact lenses under which he could imprison a bubble of oxygen or a bubble of nitrogen and then investigated the state of the cornea after some hours' wear. Oxygen maintained the cornea in a normal state, while nitrogen allowed swelling. This was a direct proof that respiration of the corneal cells is essential to maintain visual function. The corneal cells are normally covered by a film of tears, and the oxygen of the air diffuses into them through this film. By using contact lenses that have one or two small holes near the edges so that tear fluid can seep in and out with the small movements of the contact lens on the eye, the epithelial cells can get sufficient oxygen and glucose to maintain both respiration and their proper shape and, hence, their visual function.

These experiments were carried out in the living animal. Other experiments, using excised eyes or corneas, show the same dependence of normality on metabolism. The cornea will swell if it is cooled or if, at normal temperature, it is deprived of oxygen or glucose, but the swelling can be reversed when the cornea is warmed and oxygenated in the presence of glucose [4]. Unfortunately we do not know the processes involved. We know that osmotic equilibrium is linked to respiration, but that is all.

Very similar experiments have shown that oxidative metabolism of the lens is necessary to keep it optically transparent and non-swollen. The lens, enclosed within its capsule, suspended by the radially placed zonular fibres, grows throughout life by adding new fibres to the outer edges and so pushing the old fibres to the centre (figure 2). Its function is to focus the light on to the retina, and to do this it must maintain its clarity and its elasticity throughout life. The metabolic processes that go on in the lens are essential for this. Not only does it, like the cornea, gain water and sodium and lose potassium when cooled or subjected to lack of oxygen or glucose, or when exposed to metabolic poisons such as iodoacetic acid or cyanide [5], but the lens becomes cataractous when its metabolism is upset in life. Cool a mouse down, and the lens becomes opaque. Cool down the excised eye of a calf or young rabbit, and the same thing happens. Deprive a rat of oxygen, and bilateral cataracts will appear. Such lens opacities are reversible and disappear when the animal is warmed or when oxygen is restored; but most lens opacities are irreversible, and may lead to almost complete blindness.

Most of the energy of metabolism in the lens is supplied by the glucose of the aqueous humour [6]. This fluid is formed by the ciliary body, circulates over the lens, and leaves the anterior chamber by flow down the canal of Schlemm, which connects the aqueous humour with the veins, and by diffusion through the iris and cornea. Glucose reaches the lens by this circulation of fluid, and most of it is broken down in the lens fibres to lactic acid, which then diffuses away. And, as one might expect, upset of glucose metabolism is associated with formation of cataract.

Diabetic cataract is known in man, and can be produced in experimental animals by cutting out the pancreas or by injection of alloxan or dehydroascorbic acid, which induce permanent diabetes. Cataracts can also be produced within a matter of days in young rats by feeding excessive amounts of sugars such as galactose or xylose (figure 3), and a description of biochemical changes in such cataracts exemplifies the inter-relation of normal metabolism and normal function in the lens.

Let us consider the case of babies first. Some babies have an inborn error that prevents them from metabolising the galactose moiety of the lactose in milk. They do not grow well and they develop cataracts. The galactose they cannot metabolise accumulates in the blood and is excreted in the urine. This inborn error is termed galactosaemia. It is possible to locate the break in the galactose metabolic chain and to show its relevance to the biochemistry of the eye.

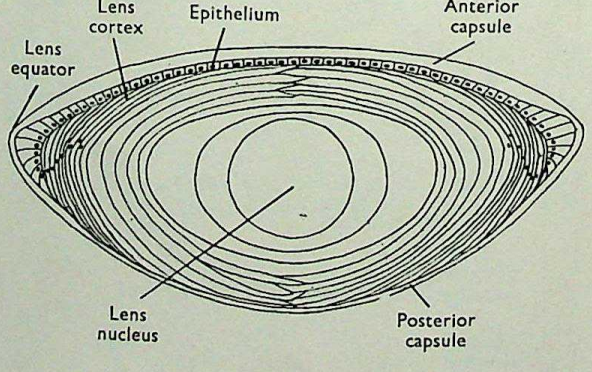

FIGURE 2 – *The lens in section.*

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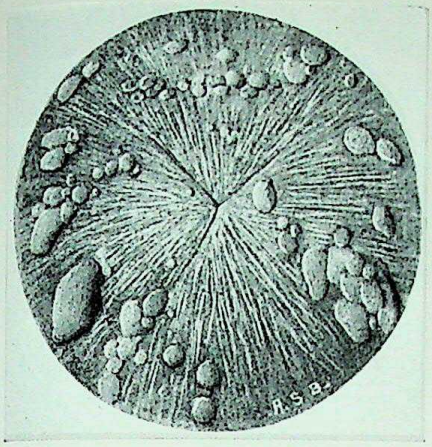

FIGURE 3 – *Galactose cataract in the rat.*

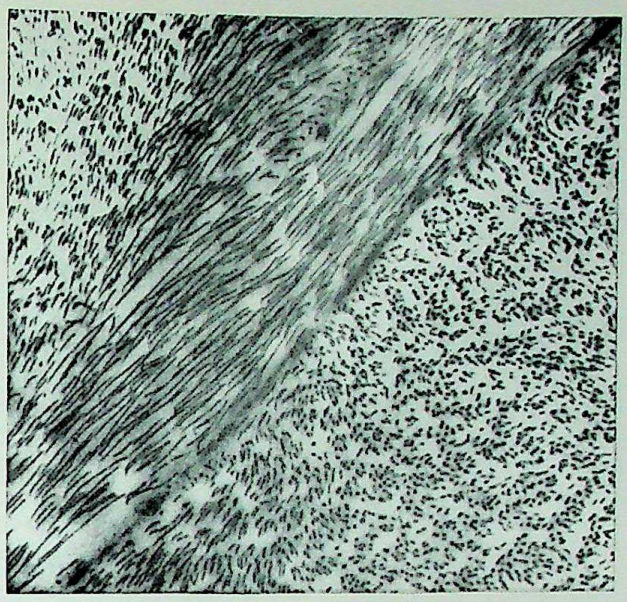

FIGURE 4 (*right*) – *Electron micrograph of interlacing bundles of collagen fibres in the cornea* [22]. (×7150)

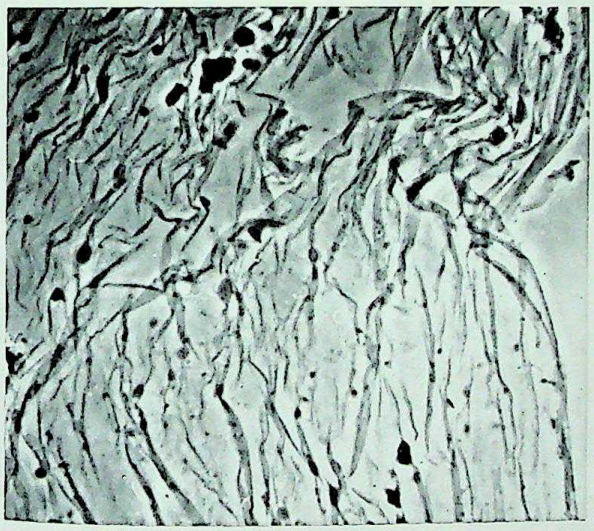

FIGURE 5 – *Hyaline membrane of the vitreous body of the ox.* (*Phase contrast,* ×700)

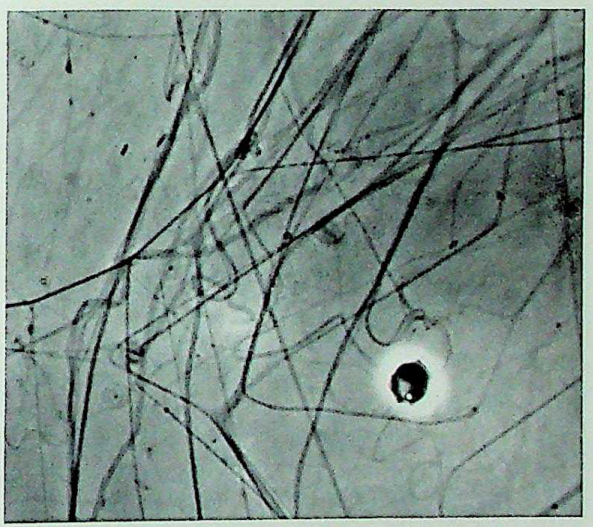

FIGURE 6 – *Coarse fibres of the vitreous body of the rabbit.* (*Phase contrast,* ×700)

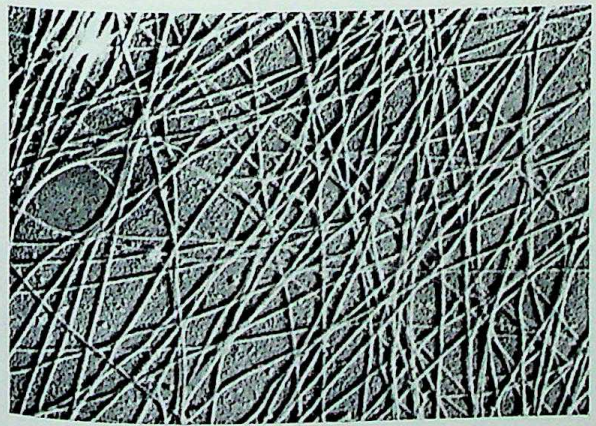

FIGURE 7 – *Electron micrograph of fine collagenous fibres of the vitreous body of the ox* [17]. (×31 000)

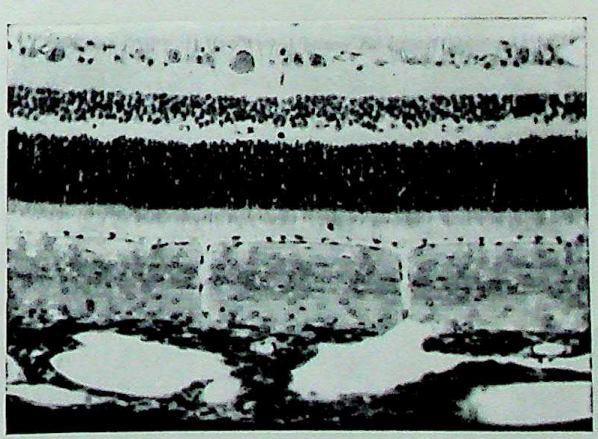

FIGURE 8 – *Section of retina of cat to show flat tapetal cells.* (×130)

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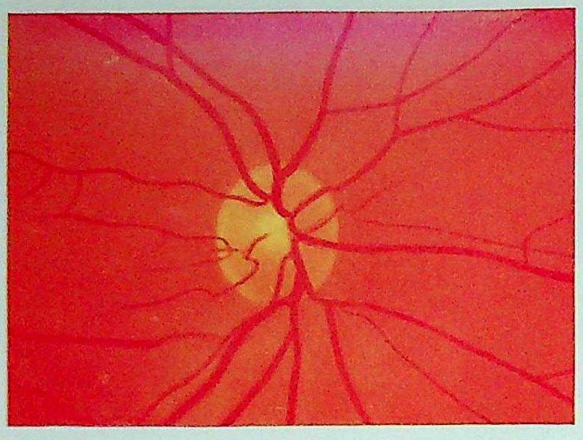

FIGURE 9 – *Human retina seen with ophthalmoscope.*

FIGURE 10 – *Cat retina seen with ophthalmoscope, showing tapetum.*

FIGURE 11 – *Opened eye of cat to show tapetum.*

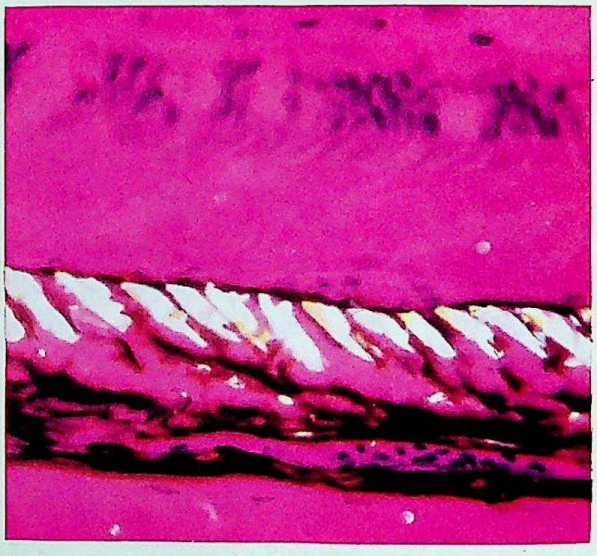

FIGURE 12 – *Guanine crystals in choroid of dogfish, viewed under polarized light.*

FIGURE 13 – *Crystals in tapetum of dog. Section viewed under polarized light.*

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FIGURE 14 – *Open eye of ox, showing retina and tapetum.*

FIGURE 15 – *Open eye of ox with retina removed.*

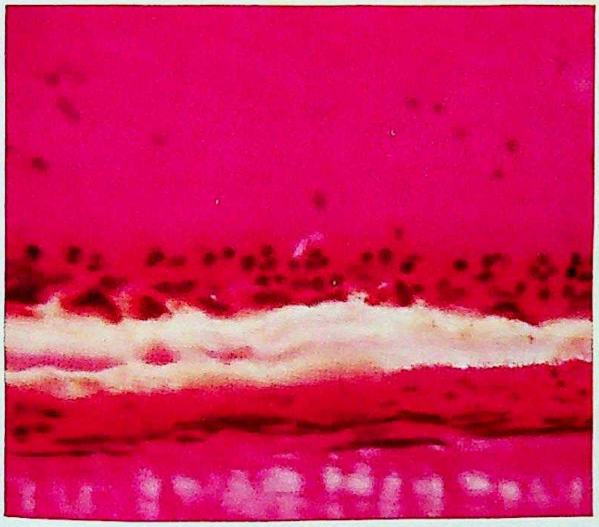

FIGURE 16 – *Guanine crystals in retinal tapetum of alligator* (Alligator Mississippiensis).

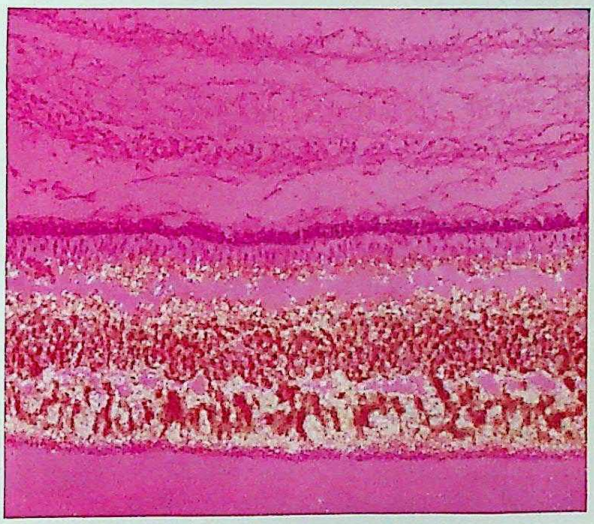

FIGURE 17 – *Guanine crystals in retinal tapetum of bream* (Abramis brama).

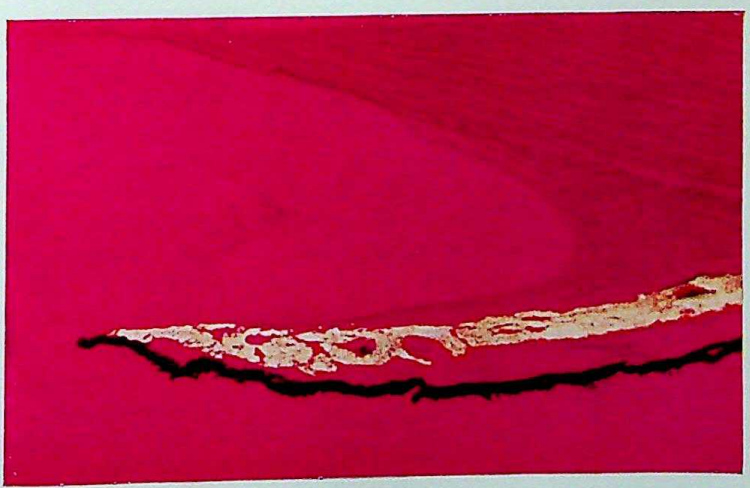

FIGURE 18 – *Section of iris of carp, showing guanine crystals on front surface, viewed under polarized light.*

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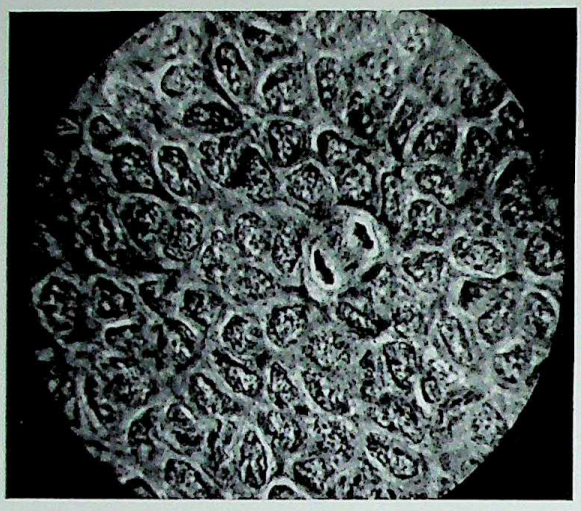

FIGURE 19 – *Normal lens epithelium, showing a normal mitosis.*

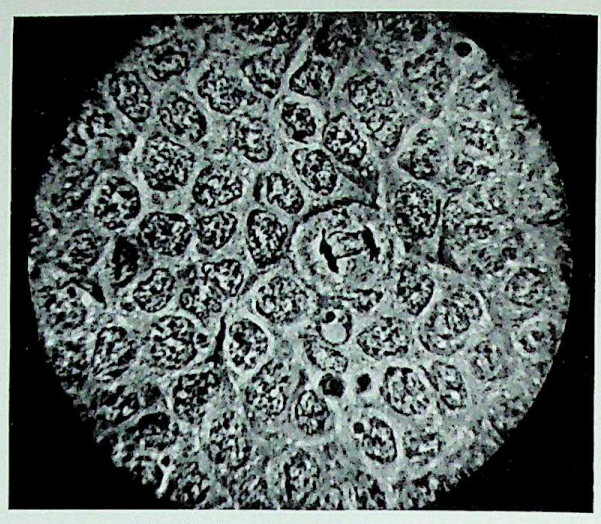

FIGURE 20 – *Epithelium from lens irradiated 14 days earlier. Note one abnormal mitosis and several fragmented cells.*

The first stages of galactose metabolism are considered to be these:

$$\text{ATP} + \text{galactose} \rightarrow \text{galactose-1-phosphate} + \text{ADP}$$

$$\text{galactose-1-phosphate} + \text{UDPG} \rightleftharpoons \text{glucose-1-phosphate} + \text{UDP gal}$$

$$\text{UDP gal} \rightleftharpoons \text{UDPG}^1$$

There is an accumulation of galactose-1-phosphate in the red blood corpuscles of galactosaemic infants [7], and haemolysed red blood cells from such infants cannot form UDP gal from UDPG and galactose-1-phosphate [8]. Galactosaemia therefore seems to be a congenital deficiency of the enzyme that catalyses this reaction. This enzyme is normally present in tissues, including presumably the lens, as galactose can be broken down to lactic acid by cattle lens *in vitro* [9].

Investigation of the metabolism of galactose in the lens of the galactose-fed rat shows that the normal lens contains only traces of galactose and galactose-1-phosphate. This is true also of the lens of the galactose-fed rat before opacity develops, but when the lens develops a cataract there is accumulation of galactose-1-phosphate in the lens capsule and epithelium [10]. It is suggested that this substance inhibits the normal breakdown of glucose by the lens and so causes development of opacity.

There are many other ways in which lens opacities may be produced in experimental animals, and metabolic studies of such partly or totally opaque lenses have shown that loss of adenosine triphosphate, loss of glutathione, and finally loss of soluble protein is a common phenomenon. But it is very difficult to know what starts off such degenerative changes. In an attempt to find out, we have studied development of X-ray cataract in rabbits, exposing one eye to the X-ray beam and using the lens of the other eye as a normal control [11–13]. No change is visible clinically in the lens for quite a long time after radiation. After our standard dose of 1400 r, the lens of a six-month rabbit may appear normal clinically for several months, though it will almost certainly be completely opaque eight months to a year after irradiation. In a younger rabbit the process is greatly accelerated; in an older one, complete opacity may never occur.

While these clinical changes are taking place it is easy to show changes in constituents and in the activity of enzymes in the lens by comparing the irradiated lens with the normal lens of the other eye. In this way we can show that there is a progressive diminution of glutathione, of glutathione reductase, and of the enzymes that synthesize glutathione from its constituent amino acids. The earliest change we have found is diminution of glutathione, and we have used this as a yardstick against which to measure other changes. There is a falling off in the activity of glyceraldehyde phosphate dehydrogenase, of aldehyde oxidase, and of glyoxalase; proteins are synthesized at a lower rate; finally there is actual breakdown of lens proteins. As with other forms of cataract,

[1] ATP = adenosine triphosphate
ADP = adenosine diphosphate
UDPG = uridine diphosphoglucose
UDP gal = uridine diphosphogalactose

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there is loss of high-energy phosphates, such as adenosine triphosphate. On the other hand, some enzymes are practically unaffected. Hexokinase, which catalyses the phosphorylation of glucose, aldolase, which splits hexose diphosphate to triose phosphates, and lactic acid dehydrogenase, which are all enzymes concerned with glycolysis, and also oxidative enzymes such as *iso*citric dehydrogenase, the malic enzyme, and cytochrome reductase, are still active in the opaque lens or lose activity at a very late stage.

But these changes in constituents and enzymes are not detectable for some time after irradiation. It may be days, weeks, or even months, depending on the age of the rabbit and the dose of radiation, before diminution of glutathione or loss of high-energy phosphate can be detected. This delay in onset suggests that these changes are secondary and that radiation is doing something we have not yet detected. In an attempt to analyse this we have begun a histological examination of the lens.

Radiation is known to damage cells capable of division more readily than others. In the lens, the dividing cells are concentrated in a ring just anterior to the equator, and it can be shown that if a beam of X-rays is passed through the core of the lens, thus missing the dividing cells, no opacity of any kind will ensue, but if the core is shielded and the whole periphery of the lens is irradiated a complete cataract will result [14]. Thus radiation damages only those cells at the periphery—probably, by analogy with other tissues, those epithelial cells capable of division. These constitute an extremely small proportion of the total lens mass, yet are of prime importance for lens function (figures 19 and 20).

A further point, which may be of general interest, has also emerged. We found that if a small part of the periphery of the lens was shielded the lens opacity produced was trivial, while if the total periphery was irradiated total opacity developed. A small area of healthy dividing epithelium therefore has a very great effect in maintaining lens structure. We are now trying to see what kind of repair process this is by irradiating one half only of the lens and microscopically examining flat preparations of the epithelium at different times afterwards in order to see whether a migration of cells from the non-injured to the injured side takes place.

All this is very far from being detailed biochemistry. Radiation injures the dividing epithelial cells; as a consequence the growth of the lens is impeded, there is actual loss of epithelial cells, and those remaining, many of them damaged, cannot maintain the inner fibres. At a certain stage these begin to decay, and at this point one can detect enzymic and other changes in the lens as a whole. But one still has to find out what the injury is in biochemical terms.

THE VITREOUS BODY

Let us now go deeper into the eye and consider the vitreous body, the last stage in the path of the light ray to the retina. This jelly fills the space between retina and lens. Mörner in 1896 demonstrated that the vitreous body of the cow contained collagen (the main fibrous protein of the skin), but this was forgotten and for many years it was taught that it was structureless. We have now come full circle and find an embarrassingly large number of diverse fibrous and membranous structures to be present, some of which resemble collagen and some of which do not (see figures 5–7) [15–17].

The vitreous body can easily be separated by filtration or centrifugation into two parts, the soluble vitreous humour and the insoluble fibrous residue. The fibrous residue of the vitreous body is complex—the main part is made up of a meshwork of very fine collagenous fibrils (figure 7) which are surrounded by a hyaline membrane (figure 5). The whole structure is anchored to the retina by coarser fibres which spring from the area of the retina just behind the lens (figure 6). The vitreous humour, which is viscous owing to the presence of a mucopolysaccharide, hyaluronic acid, permeates the whole mass.

How can one relate this structure to the behaviour of the vitreous body during life? Two troubles in the eye which affect vision, and which are directly or indirectly concerned with vitreous structure, are partial liquefaction of the vitreous body with formation of opacities, and retinal detachment.

When you look at a blank wall or a clear sky you frequently see small floating grey or black dots or threads which, if your eye is still, gently move across your field of vision. These 'floaters', as they are called, are present in nearly everyone's eyes and are little tags of fibre in the vitreous body. They move because the vitreous body is not rigid. They move very slowly and only for small distances because it is a viscous jelly.

These small fibrous knots and remnants are normal, but if the fibrous network of the vitreous body gets disorganized, either through its own partial liquefaction or through depolymerization of the hyaluronic acid, serious opacities develop.

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A still more serious threat to vision is detachment of the retina. Here a part of the retina tears or floats off and so becomes useless as a light-receiving mechanism. I think it is generally considered that at least some types of retinal tears and detachments are due to traction on the retina by vitreous fibrils. The retina is most firmly attached to the vitreous body at the base, near to the ciliary body, and it is here that retinal degeneration and detachment most frequently take place.

One might say that, both in the case of vitreous opacities and in retinal detachment, digestion of the fibres of the vitreous body by an enzyme injection might be helpful. Unfortunately collagenase, which will certainly liquefy the vitreous body both *in vitro* and *in vivo*, will also dissolve the collagen in the retinal blood vessels, and injection of collagenase into the vitreous body can cause retinal haemorrhages. Again, although collagenase liquefies the main vitreous body it does not digest the fibres which most firmly attach it to the retina at the anterior edge. These fibres (figure 6) are not collagen. It is not known what class of fibrous protein they belong to or whether they are unique. It might be that if we could digest these fibres—they are susceptible to trypsin—we might modify a retinal detachment, but until we know a good deal more of their chemical nature we cannot attempt to modify them *in vivo*. But on the whole the vitreous body behaves admirably. It is 99 per cent water, it is almost optically clear, but yet sufficiently rigid to hold the eyeball in shape and the retina in place; its chemistry is adapted to its function.

I have, of course, in writing throughout of the eye, extensively oversimplified, because there is in fact no such thing [18]. The eyes of different members of the same species differ in their details, and there are striking differences between the eyes of, say, cats, and bulls, and fish. All differ from one another, both in construction and in performance. Performance is very difficult to judge, because the eye and the brain form a unit for this purpose. A consideration of structure leads us into some very peculiar biochemistry, and to demonstrate the startling differences that exist between the eyes of different animals I can do no better than describe the chemical adaptations which are the basis of eye 'mirrors'.

ADAPTATIONS IN CHOROID AND RETINA TO FORM A REFLECTING SURFACE

When one is out at night with a torch or in a car one can often see the glowing eyes of a cat or dog reflecting the light back at one. Many mammals and fishes reflect light in this way. Birds' eyes in general do not, nor do ours. In our eyes the retina is backed by a uniformly black choroid which simply absorbs all light which reaches it. Light reflected from the human retina is reddish and comes from the retina itself and its blood vessels (figure 9). But many animals (figures 11, 14, 15) have a special area of choroid or retina, called a tapetum lucidum, which acts as a mirror, so that light which falls on it is reflected through the overlying retinal cells again [18]. Obviously this cannot increase visual acuity but could be useful in raising the sensitivity to dim light, since any light not absorbed on its first passage through the retina might stimulate the light-sensitive cells as it traversed them again after reflection.

The tapetum lucidum in the dog or cat is a triangular area in the upper half of the eye which, when viewed in life (figure 10) or in the excised eye (figure 11), appears as a blue-green glistening area. In microscopical section it can be seen that the pigment epithelial cells of the retina contain no pigment in the tapetal area and are backed by several layers of large flat cells in the choroid (figure 8). These cells are full of crystals (figure 13). Recently it has been found that the crystalline material laid down in the tapetum of carnivores such as the dog, cat, badger, seal, and fox is a complex of zinc with the sulphur-containing amino acid cysteine [19]. The cells of the tapetum of the silver fox contain 16 per cent of their dry weight as zinc and 8–9 per cent as sulphur.

That this tapetal area is essential for visual function is suggested by the fact that if a dog is injected with a metal chelating agent such as dithizone the eye shine is abolished, the animal becomes blind as the tapetum degenerates, and the retina becomes detached. Dithizone is frequently used to produce diabetes, as it causes degeneration of the islets of the pancreas (again probably owing to removal of zinc), but the dose required to cause visual changes in dogs is lower than the dose necessary to produce diabetes. No repair seems to take place in the eye after removal of zinc cysteine [20].

Many fish, amphibia, and reptiles also lay down crystalline material, not only in their choroid but also in their iris (figures 16–18), where again it acts as a reflector. These crystals are made up of the purine guanine, which is also the substance that gives fish scales their silvery appearance; it is indeed known as 'fish silver'. In the dogfish, *Squalus acanthias*, the guanine crystals are

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very large and arranged in a regular way, apparently at an angle of 45° to the visual cells (figure 12). In dim light they are uncovered, but in bright light they become covered with pigment which migrates to and fro according to the light, thus increasing the range of light intensities that the eye can respond to.

Herbivores such as cattle and sheep have modified their choroid rather differently, but also to form a mirror. In these animals the choroidal tapetum is made up of fine parallel fibres which act as a diffraction grating and reflect coloured light in the same way as the wings of some butterflies. The choroid has a high concentration of barium, a metal which is practically absent from all other parts of the body but which may reach 120 mg/100 g dry weight in the choroid of the cow [21]. We do not know whether the barium is connected with the tapetum.

The basic mechanism of vision lies in the light sensitivity of the pigments in the rods and cones of the retina. Added to this there is the interaction between eye and brain so that the image received on the retina may be interpreted. Optically the eye of the bird is in many ways superior to the eye of man, but one can hardly doubt that the range of visual experience of the bird is less than that of man owing to the relative simplicity of its brain.

The whole eye is adapted to the purpose of forming a clear image on the retina. We have seen that the efficiency of the light-transmitting and light-focusing parts of the eye—the cornea, the lens, and the vitreous body—is dependent both on metabolism and chemical structure, and we have seen that extraordinary chemical specializations take place in the choroid in order to increase the range of light intensity to which the retina can respond. Each separate structure in the eye has a part to play, and normal light reception depends on the normality of each part.

ACKNOWLEDGMENTS

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Some recent advances in molecular biology

M. F. PERUTZ

Molecular biology seeks to explain biological function in terms of molecular structure. Great strides have recently been made, and it is now widely expected that the simplest forms of life will be understood in molecular terms within the next twenty years. The key problems are the structure of the nucleic acids, on which genetic replication and protein synthesis depend, and the structure of the protein catalysts which control metabolic activity. This article discusses the structure and function of nucleic acids. It describes the first three-dimensional model, obtained by X-ray analysis, of a protein molecule and tentative models of small viruses. It also shows how genetic mutations act by altering the constitution of protein molecules.

GENES AND ENZYMES

The bewildering variety of living forms conceals an underlying unity in the molecules that nature uses for the processes of reproduction, multiplication, growth, movement, metabolism, secretion, and nervous response. Similar enzyme molecules catalyse the same reactions in unicellular organisms and in mammals; some of the same co-factors are probably at work in glycolysis and in photosynthesis; the same few simple molecules are used in the biosynthesis of the many complex organic compounds found in living organisms. Almost all metabolic processes are catalysed by enzymes and all enzymes are protein molecules. By teaching this creed twenty-five years ago Sir Frederick Gowland Hopkins created the scientific climate in which our research on the chemical structure and spatial architecture of proteins was begun.

In recent years biochemists have started to probe deeper, asking themselves what controls the synthesis of enzymes. It cannot be other enzymes, since they would have to be made by yet more enzymes and so on *ad infinitum*. We now know that enzyme synthesis is controlled by genes and we have good reason to believe that one gene controls the synthesis of one enzyme. A gene must therefore possess a dual function; it must be able to replicate itself and to determine the specific structure of a protein molecule. This poses the question of the chemical nature of genes.

It would appear the simplest hypothesis, at any rate at first sight, to suppose that enzymes were self-reproducing molecules in addition to being functional catalysts. However, recent research on viruses and bacterial transforming factors has given convincing proof that the essential part of a gene is made not of protein, but of nucleic acid. This may be of two different kinds: deoxyribonucleic acid (DNA) in cellular organisms and large viruses, or ribonucleic acid (RNA) in certain small viruses. Biological replication and metabolic activity therefore appear to depend primarily on the structure and interaction of two types of very large molecules: nucleic acids and proteins. The part played by lipoids and polysaccharides is not yet clear; at present it seems to be secondary.

The basic questions that biologists have long been asking, therefore, are these. What is the structure of the genetic material? How does it replicate itself? How does it control the synthesis of enzymes? What structures do enzymes have, and how do the structures determine their catalytic function?

THE STRUCTURE AND REPLICATION OF THE GENETIC MATERIAL

We are used to thinking of enzymes as molecules, but the idea of the gene as a molecule with a definite stereochemical structure is relatively new. We think of genes in an abstract way as unit factors of heredity which are arranged in a linear order along the chromosomes, or perhaps the term gene conjures up a picture of the banded structures that can be seen in the giant chromosomes of the fruit fly. Chemically, chromosomes consist of DNA and protein; RNA occurs at some stages of the mitotic cycle, but since it is absent in sperm, it can safely be excluded as a prime carrier of hereditary information in higher organisms. The idea that it is the DNA alone that carries the genetic information gained validity with O. T. Avery's discovery of bacterial transforming factor and has become widely accepted only since A. D. Hershey and M. Chase demonstrated DNA to be the main infective component and the carrier of genetic information in bacterial viruses.

It might be argued that the hereditary apparatus of higher organisms need not necessarily be

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the same as that of microorganisms. This may be true, and indeed there is as yet no direct proof of the part played by nucleic acid in the chromosomes of higher animals. However, the constancy of the DNA content and composition in all cells of a given species, its metabolic inertness, and a variety of other indirect evidence make us believe that DNA is in fact the sole carrier of genetic information in all organisms except the small viruses, where its place is taken by RNA. It is sometimes argued that an entirely new class of compound may yet be discovered that might revolutionize our ideas on this matter, but there is at present no experimental basis for this argument.

If the nucleic acids are to convey genetic information they must be so made as to form a chemical code. They are in fact long-chain polymers, with a backbone in which an identical chemical pattern repeats at regular intervals, forming the links of the chain (figure 1). Attached to each

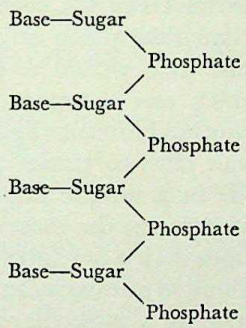

FIGURE 1 – *Chemical structure of DNA. The compound consisting of one phosphate, sugar, and base is called a nucleotide.*

link is a purine or pyrimidine base. In any one nucleic acid these bases are of four different kinds. We do not yet know in what sequence the bases are arranged in any nucleic acid, nor do we have any direct information that they are arranged in any definite sequence at all; we know only that their proportions are characteristic and constant in the DNA of any particular species. It is the fact that the bases are the only variable constituents that makes us believe that they are arranged in a definite sequence and that this sequence constitutes the genetic code. If this is true, then the genetic language is written in a four-letter alphabet on an immensely long scroll. The actual length of the scroll is about 0·003 mm in a small plant virus, 0·1 mm in T2 bacteriophage, and 1000 mm in a mammal, i.e. many times the length of the virus or cell that contains it. The number of nucleotide letters which these lengths correspond to are 8000, 500 000 and 3000 million respectively, the last equivalent to a great library of information.

Each time a parent cell divides, an exact copy of the genetic information has to be made in order to ensure continuity of inheritance in the daughter cells. We do not yet know how this is done, but J. D. Watson and F. H. C. Crick have proposed a structural model of DNA that suggests some features of a possible mechanism [1]. Their model is of special interest in this context because it shows most clearly how a new concept of molecular structure can give birth to new ideas of biological function, and may even eventually lead to the interpretation of pathological changes.

The Watson-Crick model of DNA consists of two chains that run in opposite directions and are coiled around each other to form a double helix (figure 2). It looks like a spiral staircase in which the bases form the steps and the phosphate ester chain provides the banisters. Each step consists of two bases, one from each of the chains, which are linked together by hydrogen bonds. Suppose now the four different types of bases which form the letters of the genetic alphabet are called A, T, G, and C, then the model predicts that only specific pairs of bases can be linked to form a step,

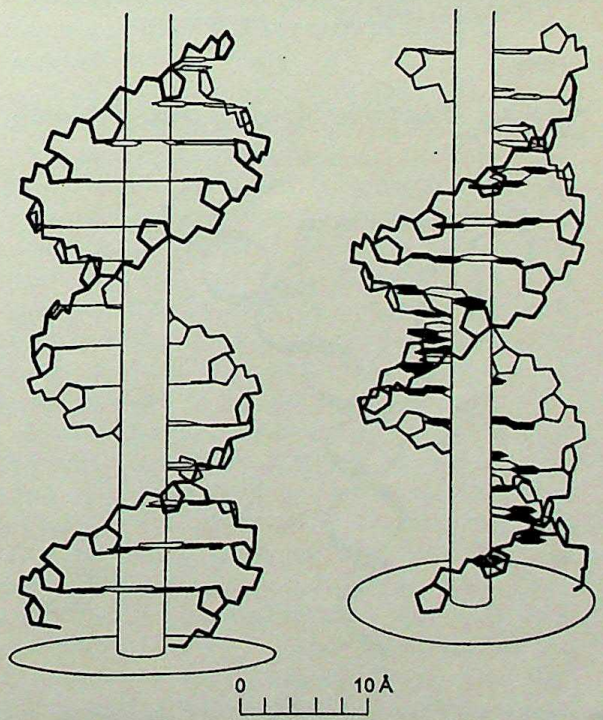

FIGURE 2 – *The double helix of DNA. The rings and wire links signify the phosphate ester chain. The plates between them represent the bases.* (*Reproduced by permission from F. H. C. Crick and J. D. Watson*, Proc. roy. Soc., A, 223, 80, 1954.)

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such that A is always linked to T and G to C. This implies that the sequence of bases in the two chains making up the double helix must be complementary, so that the sequence on one chain determines that on the other (figure 3).

When we want to copy a document we prepare a negative from which we make a positive print. The complementary sequence of bases in the two chains makes DNA a duplex of positive and negative. To reproduce itself, the two components would have to separate and each would have to make a print of itself. In this way two copies would be formed, each carrying information identical to that of the parent duplex (figure 4).

By focusing people's ideas on a definite structure and a mechanism of replication, the Watson-Crick model has acted as a stimulus for a variety of interesting experiments. The model owed part of its inspiration to the X-ray diffraction work of M. H. F. Wilkins and R. Franklin [2]. These two workers and their associates have since proved that the X-ray diffraction patterns of DNA fibres, from whatever source, are fully consistent with the double helix of Watson and Crick, though certain features of the initial model need modification. Wilkins also proved the double helix to be present, not only in purified DNA fibres, but in live sperm heads, possibly with a third chain, of protamine, wound around it. We can be fairly sure, therefore, that the structure is right and that it occurs in live chromosomes.

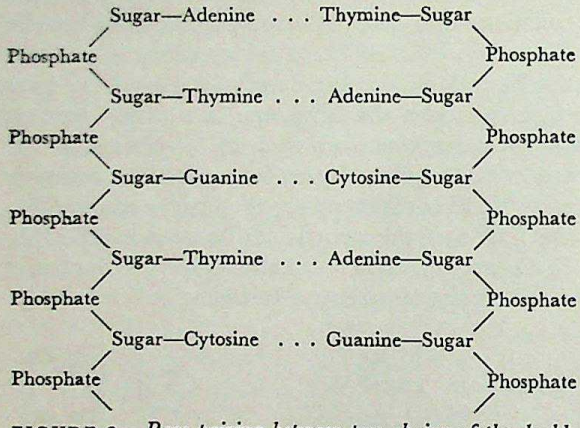

FIGURE 3 – *Base pairing between two chains of the double helix of DNA.*

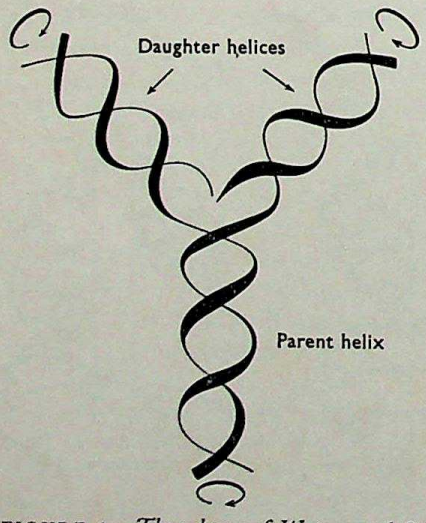

FIGURE 4 – *The scheme of Watson and Crick for the self-replication of DNA. Unwinding of the parent duplex, and replication, and rewinding of the duplicates would proceed simultaneously. All three arms of the* Y *would rotate as indicated. (After M. Delbrück and G. S. Stent, by courtesy of the Johns Hopkins Press.)*

An essential feature of the DNA code is its linearity. This prediction has been tested by S. Benzer, at Purdue University, in a most ingenious experiment on the genetic recombination of bacteriophage. The results of his work have shown that mutations can take place at as many as a hundred different sites within a single physiological gene, which can therefore no longer be regarded as an indivisible unit (figure 5). Moreover, Benzer was able to prove that all these mutating sites lie in a unique linear array, and that the distance between the most closely spaced loci can be no more than a few links in the nucleotide chain [3]. Thus, in one organism, the linearity of the genetic code on a molecular scale has now been verified experimentally.

We now come to the mechanism of replication.

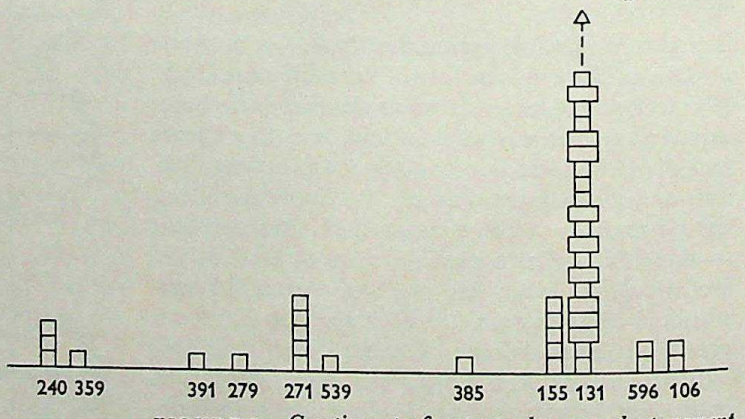

FIGURE 5 – *Genetic map of mutants along one short segment of one of the genes in T4 bacteriophage. Each box signifies one occurrence of the mutant. The distances between the mutants are based on recombination frequencies between them and have no direct meaning in physical terms, but rough calculations indicate that the physical distance between the most closely spaced mutants corresponds to a few links along a DNA chain. (After S. Benzer, by courtesy of the Johns Hopkins Press.)*

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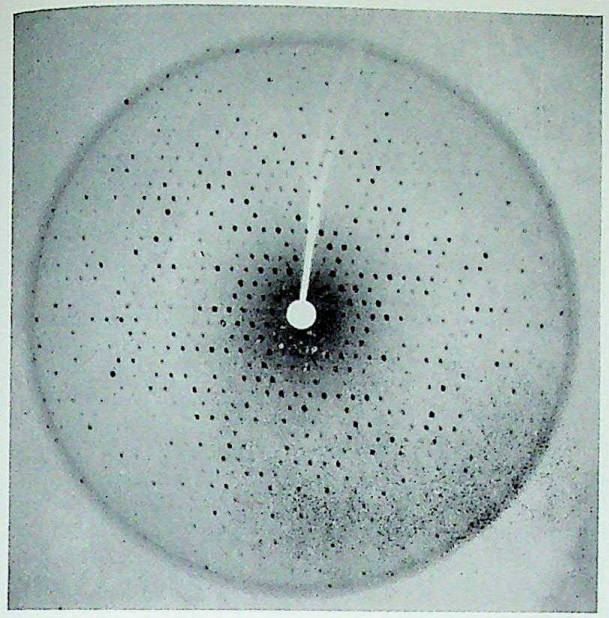

FIGURE 6 – *X-ray diffraction photograph from a single crystal of haemoglobin. Each spot is a diffracted image of the crystal. The different intensities of the spots provide the information from which the structure is ultimately deduced.*

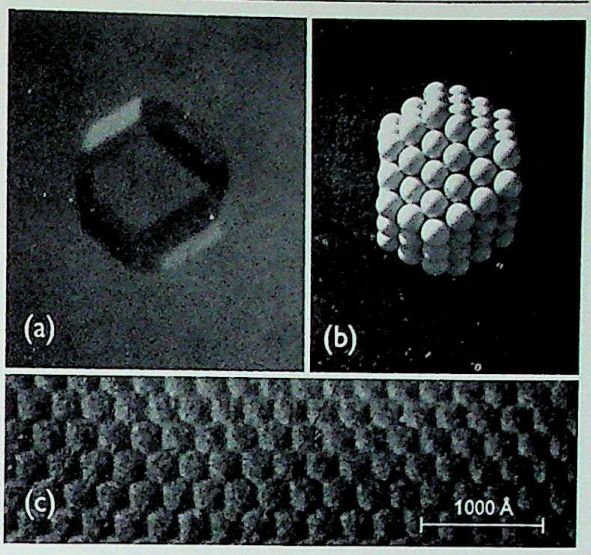

FIGURE 8 – (a) *Photomicrograph of crystal of poliomyelitis virus (Mahoney strain).* (b) *Ball model showing probable arrangement of virus particle.* (c) *Electron micrograph of virus particles regularly packed on one of the crystal faces. (Reproduced by permission from R. L. Steere and F. L. Schaffer,* Biochem. Biophys. Acta, **28**, 241, 1958.)

Watson and Crick's model suggests that in each cycle of replication one of the two parent strands of the double helix should go to each of the new double helices (figure 7). This prediction can be tested by labelling the parent DNA with isotopes and then determining the distribution of the labels in the DNA of the progeny. Several ingenious experiments of this type have now been done; one on bacteriophage, one on *E. coli*, and the third on the chromosomes of bean seedlings. Each has given an answer consistent with the mechanism of replication indicated in figure 7, though two of the experiments leave some doubt whether the transferred label is really part of the DNA that carried the genetic code.

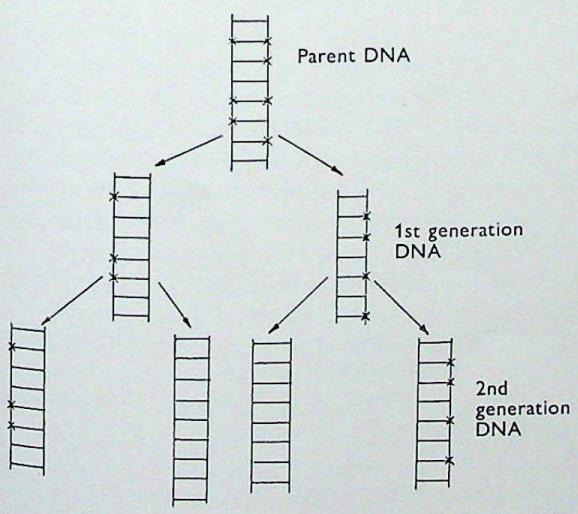

FIGURE 7 – *Distribution of isotopic label of parent DNA among double helices of progeny DNA. The crosses indicate isotopic markers.*

On the chemical side, a cell-free enzyme system has been isolated from *E. coli* which synthesizes DNA, provided all four bases are present among the nucleotide precursors. A further condition for synthesis is the presence of a primer, and this primer has to be DNA. These conditions are in accordance with expectation if synthesis of a DNA chain requires the presence of a complementary chain on which it can grow, like a bindweed, and if growth would be interrupted if one of the four bases necessary for pairing were absent.

It would be too early to say that the mechanism of genetic replication suggested by the Watson-Crick model has been proved conclusively, but supporting evidence is steadily accumulating. On the other hand, there are still two important aspects of replication which are far from clear, and which any general theory ought to be able to explain. One is the origin of mutations and the other is the mechanism of crossing over. The model suggests that the simplest mutation should consist in a replacement of one base pair in the DNA chain by another, say A–T by G–C; the action of certain chemical mutagens is indeed consistent with this idea. However, there is the

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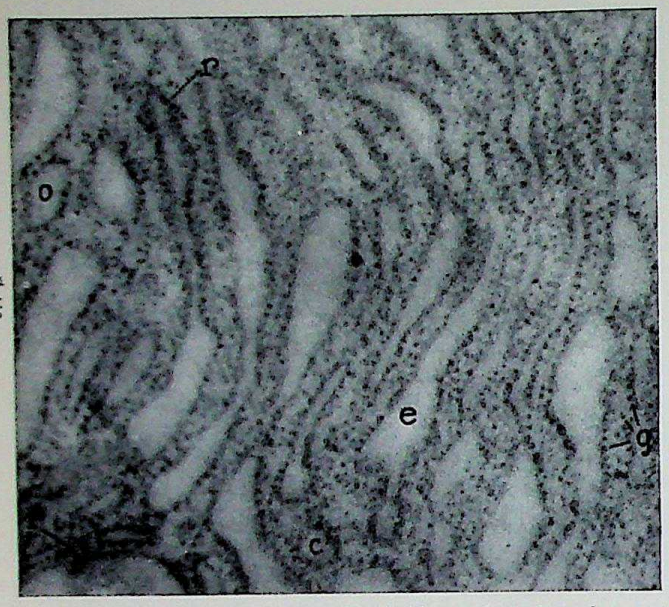

FIGURE 9 – *Electron micrograph of thin sections through a rat pancreas. The field is taken up by structures known as endoplasmic reticulum (e, c, and o) whose matrix contains many small dense granules (g and r). These are the microsomal particles which synthesize proteins. (Reproduced by permission from G. E. Pallade,* J. Biophys. Biochem. Cytol., 1, 68, 1955.)

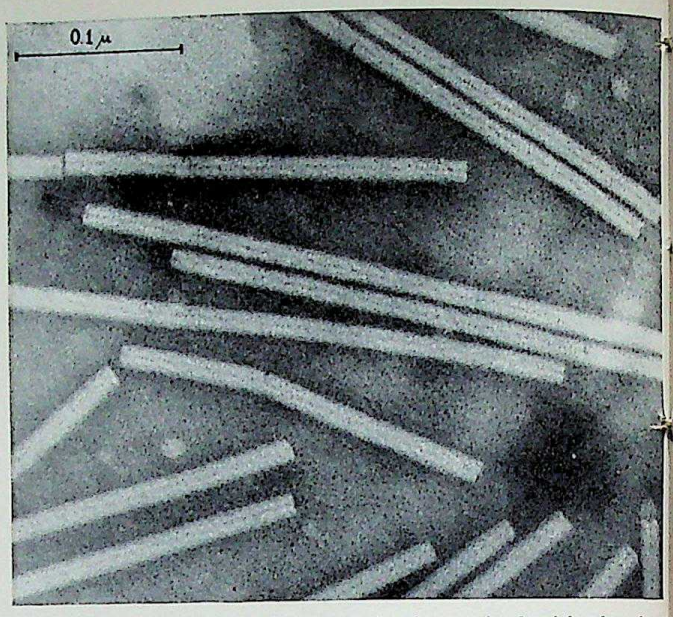

FIGURE 10 – *Rods of tobacco mosaic virus stained with phosphotungstic acid which fills the central hole and shows it up as a dark line.* (0·1 μ = 1000 Å.) (*S. Brenner and R. Horne.*)

further prediction that the probability of such a replacement occurring should be the same anywhere along the chain. This is not borne out by Benzer's results, which indicate the frequency of mutation to be unequal at different genetic sites.

So far as I know, no plausible working hypothesis has yet been produced for a molecular mechanism of crossing over. Nevertheless, the multitude of interesting experiments now being carried out on the replication of bacteriophage, on the transformation and transduction[1] of bacteria, and on the genetics of many kinds of microorganisms are steadily adding to our understanding of genetic events. It is not too rash to predict that the structure and replication of the genetic material may be understood in as much detail in ten years' time as, for instance, the biosynthesis of fatty acids is today. Analysis of the processes in the mitotic cycle of multicellular organisms may follow the fundamental research on the molecular structure of cell constituents and should not be regarded as beyond our reach.

[1] Transformation is a genetic change in one strain of bacteria induced by the transfer of the DNA extracted from another strain. Transduction is the transfer of genetic characters from one strain of bacteria to another through infection by lysogenic phages.

In this account I have left on one side the structure of RNA because it is still unknown and because it acts as a carrier of genetic information in only the small viruses. However, as will be seen in the following section, it is RNA and not DNA that directly controls protein synthesis; RNA may also act as a carrier of genetic information from the nucleus to the cytoplasm, and its structure is therefore of supreme interest. Unfortunately, fibres of RNA have so far given too poor an X-ray diffraction pattern to serve as a basis for a structural model. The problem is therefore being studied indirectly. S. Ochoa has isolated an enzyme which synthesizes RNA-like fibres of variable composition from simple precursors. A. Rich has shown that many synthetic analogues give better X-ray pictures than natural RNA and provide valuable information about the kinds of structures which RNA might form under various conditions, such as double and triple helices. It looks as though the solution of the RNA structure is not too far distant [4].

STRUCTURAL ASPECTS OF PROTEIN SYNTHESIS

Having discussed the structure and replication of the genetic material, we can now consider its

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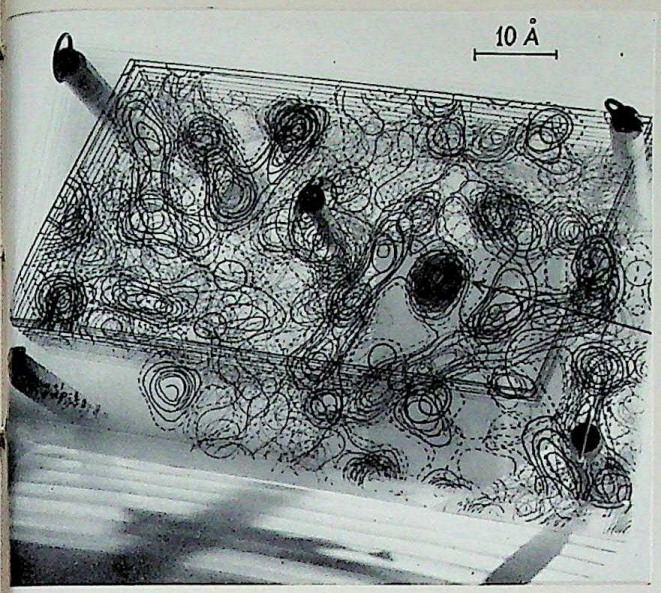

IGURE 11 – *Three-dimensional map showing the distribution of nsity along a series of parallel sections through two neighbouring olecules in a crystal of myoglobin. The arrow points at the high peak presenting the iron-containing haem group. (After J. C. Kendrew.)*

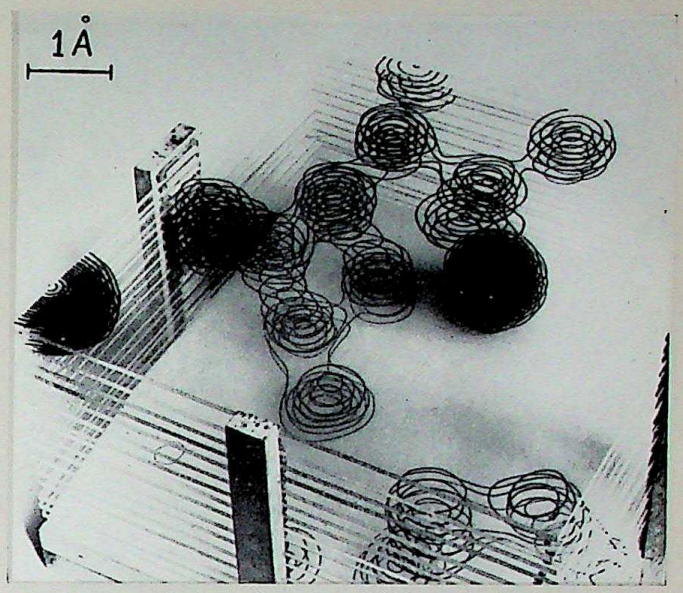

FIGURE 12 – *Sections through penicillin molecules, in which the atoms show up as peaks of high electron density. Note the difference in scale between figures* 11 *and* 12. *(After D. Crowfoot* et al, *by courtesy of the Princeton University Press.)*

function. If a gene determines the specific structure of an enzyme, then the meaning of the genetic code would be expressed in terms of protein structure. This question can be treated in a purely formal manner, regardless of the actual chemical mechanism involved in protein synthesis.

Consider the structure of a protein in relation to that of a nucleic acid. The unique feature that the two substances have in common is a specific linear sequence of different units on a long chain. What distinguishes them is the number of different kinds of unit which each contains. Any one nucleic acid contains mainly four different kinds of bases, whereas proteins contain mainly twenty different kinds of amino acids, and it is the sequence of these which determines their specificity. We now assume as a working hypothesis that the sequence of the four bases along the nucleic acid chain determines the sequence of amino acids along the protein chain, and that this is the real purpose of the genetic code.

These formal ideas suggest that the actual apparatus for synthesizing proteins is a nucleic acid template on which the amino acids are assembled in the right order before they are linked together to form a polypeptide chain. From what has been said so far, one would naturally expect these templates to occur in the cell nucleus and it is surprising that the rate of protein synthesis in the nucleus is low. Nevertheless, most protein is probably made in the cytoplasm, where the point of polymerization seems to have been tracked down to small spherical particles which have been named microsomal particles (figure 9). They have molecular weights of the order of a few millions and consist of only RNA and protein. There is evidence that it is the RNA in these particles which forms the templates on which protein chains are made [5].

It will now be apparent why it is so important to solve the structure of RNA. We hope that knowing the structure of the RNA template will help us to guess the mechanism by which the amino acids are assembled and joined. Great strides have been made in the last few years in elucidating the chemical mechanism by which amino acids are activated and brought to the assembly line.

M. Hoagland and others have discovered activating enzymes that join amino acids to adenosine monophosphate (AMP). Hoagland has also shown that these activated amino-acid-AMP-anhydrides are probably attached to soluble RNA of relatively small molecular weight as a carrier system before entering the microsomal particles [6]. It now looks as though the cytoplasm

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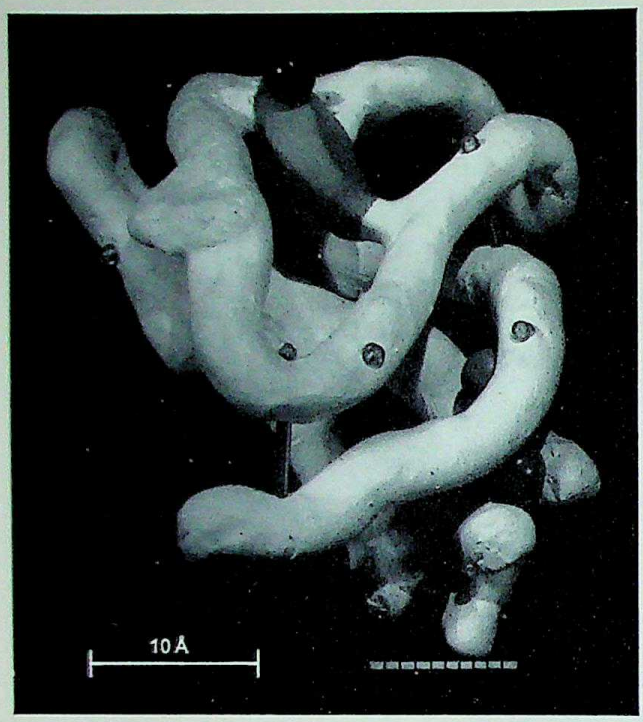

FIGURE 13a – *Model of the myoglobin molecule. The white sausage represents the polypeptide chain; the grey disk is the haem group. The little balls represent the positions of different heavy atoms used for determining the structure. The scale is in Ångström units.*

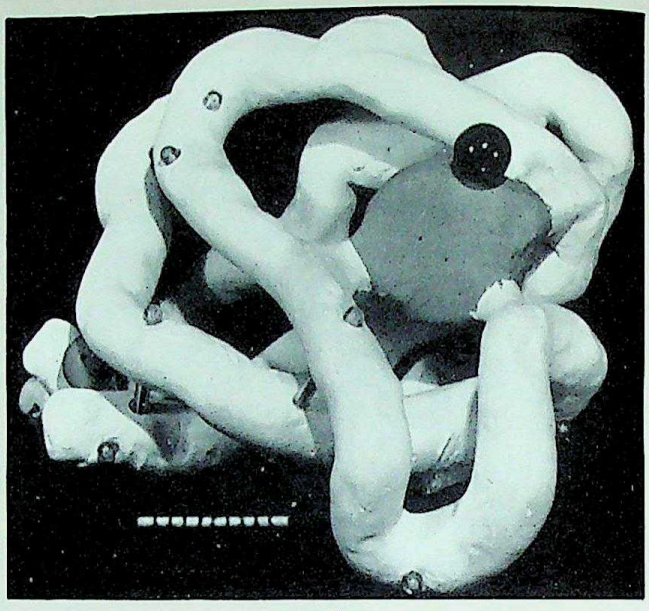

FIGURE 13b – *Top view of the molecule shown in figure* 13a [10].

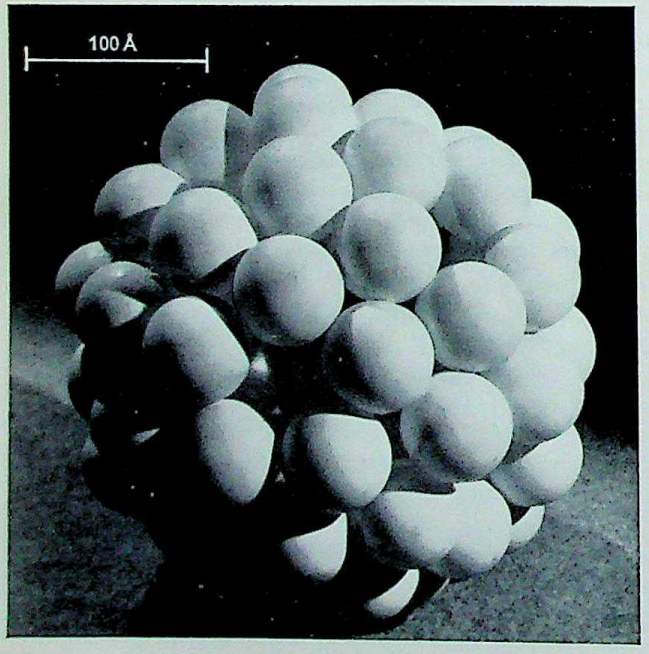

FIGURE 14 – *Model of symmetrical packing of* 60 *identical sub-units on the surface of a sphere (F. H. C. Crick). This might represent the structure of the protein shell of certain plant viruses or perhaps of poliomyelitis virus. The RNA is not shown.*

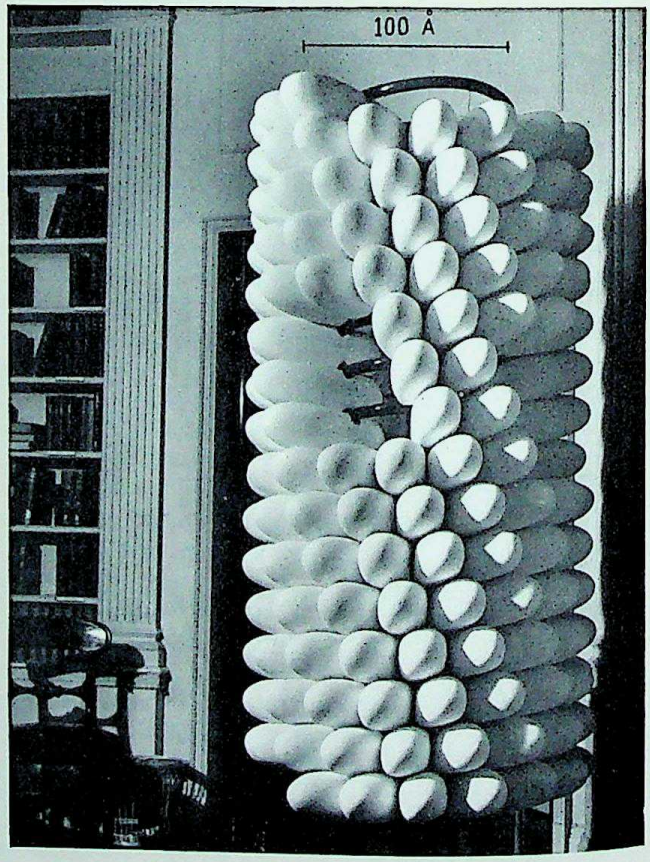

FIGURE 15 – *Model of tobacco mosaic virus. Each 'loaf' represents a protein sub-unit of molecular weight* 17 000. *The opening shows the coils of RNA inside the particle. The pitch of the coil is* 23 *Å. (Rosalind Franklin and A. Klug.)*

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contains at least two different kinds of RNA, one having a metabolic function and the other serving as a template for protein synthesis. The time may not be far off when a fusion of chemical and structural ideas will lead to the discovery of the synthetic mechanism. On the other hand, we do not yet know how the genetic information is transferred from the DNA in the nucleus to the RNA in the cytoplasm, and this is one of the important questions for the future.

STRUCTURE OF PROTEINS

Like the nucleic acids, the proteins are giant molecules. They consist of polypeptide chains in which the same chemical groups

—CO—RCH—NH—CO—RCH—NH—

repeat at regular intervals. Specificity is provided by the side-chains (R) of the twenty different kinds of amino acids (figure 16).

An enzyme molecule may consist of one or more such polypeptide chains which, together, may contain between a hundred and several thousand amino acid residues, all arranged in a definite sequence. The number of chains and the sequence of residues within them constitute the primary structure of a protein.

As in nucleic acids, the secondary bonds of the various groups in a polypeptide chain can be best satisfied if the chain is coiled into a helix. The α-helix of Pauling and Corey (figure 17) is the most stable, but not the only, kind of helical configuration that the chain can assume. The kind of helix formed or, more generally, the way one residue is related to its neighbours along the chain, constitutes the secondary structure of a protein.

From what has been written so far the reader might conclude that protein molecules are thin threads or ropes. Collagen, hair, and some muscle proteins, for example, do indeed have a structure of this kind, but the majority of enzymes are probably spheroidal in shape. We must infer that the chains or helices in them are somehow folded backwards and forwards so that the complete molecule behaves in solution like a ball. This kind of folding constitutes the tertiary structure of a protein.

Many enzymes contain one or more non-protein (prosthetic) groups that form the site of their catalytic activity. Their exact function generally depends on combination with a specific protein.

To complete this introduction to protein structure one further important point should be made. Not only is the number of polypeptide chains and sequence of amino-acid residues in a protein definite and specific, but the same is true of its secondary and tertiary structure. In fact, a protein molecule is like an animal in having a

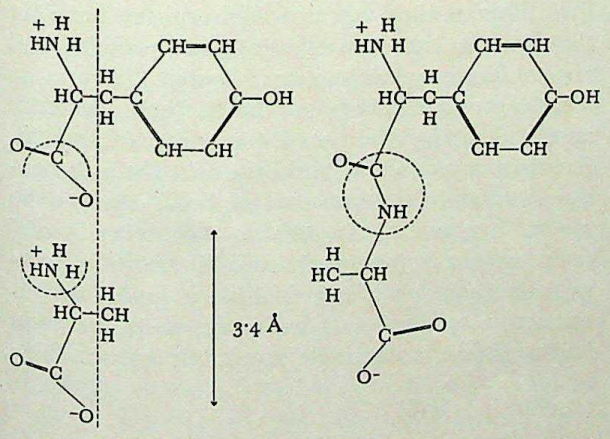

FIGURE 16 – (*left*) *Two amino acids, tyrosine above and alanine below. The part to the left of the broken line is the same in all amino acids and is incorporated in the main chain. The groups on the right form the different side-chains.* (*right*) *The two amino acids joined by a peptide bond. This leaves free the amino group at the top and the carboxyl group at the bottom to form peptide links with further amino acids until a long chain is formed.*

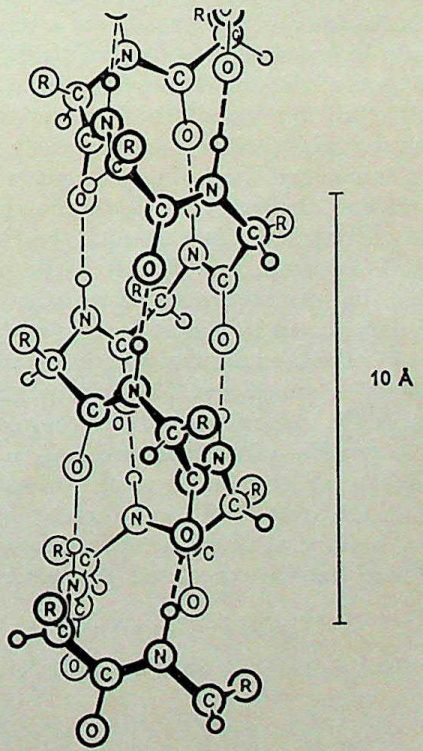

FIGURE 17 – *The α-helix of Pauling and Corey. The atoms marked R represent side-chains.* (*By courtesy of L. Pauling, R. B. Corey, and H. R. Branson,* Proc. Nat. Acad. Sci. Wash., 37, 207, 1951.)

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three-dimensional anatomy laid out to a definite plan, rigid in some parts and flexible in others, with perhaps some minor variations in different individuals of the same species.

By chemical and kinetic studies a great deal has been discovered about the catalytic site of certain enzymes, and the nature of the reactions they catalyse, but no detailed knowledge yet exists about the structure of any enzyme and not even the most rudimentary information about the great majority of them. The problem really requires a twofold approach: a chemical one, to find the number of chains, the sequence of residues, and the nature of the bond between protein and prosthetic group; and a physical one to discover the way the polypeptide chains are coiled and folded and the nature of the secondary chemical bonds which hold them together. Both approaches have been developed by members of the Medical Research Council's staff at Cambridge, one at the Department of Biochemistry by F. Sanger and his colleagues and the other by the Molecular Biology Unit at the Cavendish Laboratory. Both methods are laborious in the extreme.

Sanger's chemical approach reached its climax with his determination of the constitution of insulin, which contains 51 residues arranged in two closely linked chains (figure 18) [7]. His discovery was one of the milestones in protein chemistry. First of all it removed the last shadow of doubt from the polypeptide hypothesis which Hofmeister had enunciated more than fifty years earlier. It established the fact that the amino-acid residues really are arranged in a definite, genetically determined sequence, but disproved the widely held belief that this sequence was regular. It revealed the part played by cystine bridges in the architecture of protein molecules, and the chemical nature of species specificity. Most important of all, Sanger demonstrated that the complete formula of a protein can be determined by chemical methods, at least as far as the primary covalent bonds are concerned, and thereby stimulated a great volume of new research all over the world.

Since Sanger completed this work two years ago, he and others have tried to improve the methods of sequence study and to apply them to larger proteins, but the difficulties of fractionation and analysis rise rapidly with the size of the protein and each analysis of even a small protein is expected to take a team of research workers several years. On the other hand, the number of known enzymes is over 500 and most of them have molecular weights many times larger than insulin, which shows that the development of more rapid and sensitive methods is vitally important.

The physical approach to the structure of crystalline proteins is based on X-ray analysis. It involves studies of the X-ray diffraction patterns, generally from single crystals, and is a technique that has been widely used to determine the atomic arrangement in organic compounds. Most of these compounds, however, were at least a hundred times smaller than protein molecules, and it was a matter of great difficulty to extend the methods of X-ray analysis to molecules of such enormous size [8]. None of the approaches yielded much information until the writer discovered, in 1953, that the problem could be solved by studying the X-ray diffraction pattern from an isomorphous pair of crystals, one containing the protein alone and the other a complex of the protein with a heavy atom such as mercury. Soon afterwards it became apparent that one isomorphous pair would not be enough and that a whole series of isomorphous heavy atom compounds would be needed, each compound having a heavy atom attached to a different site on the protein molecule [9]. This method has now become the basis for the structure analysis of crystalline proteins and viruses in many laboratories (figure 6).

The greatest success of the method has been achieved during the past few months, when J. C. Kendrew was able to build the first three-dimensional model of a protein molecule ever to be made. It represents myoglobin, a protein of molecular weight 17 000 consisting of one long polypeptide chain with 153 residues in unknown sequence, plus one haem molecule, the same as in haemoglobin, as a single prosthetic group. Like

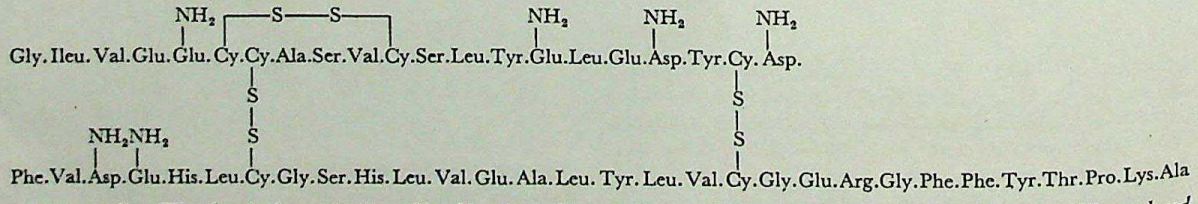

FIGURE 18 – *The chemical constitution of beef insulin. Gly. = glycine; Ileu. =* iso*leucine; Val. = valine; etc. (Reproduced by permission from A. P. Ryle, F. Sanger, L. F. Smith, and R. Kitai* [7].)

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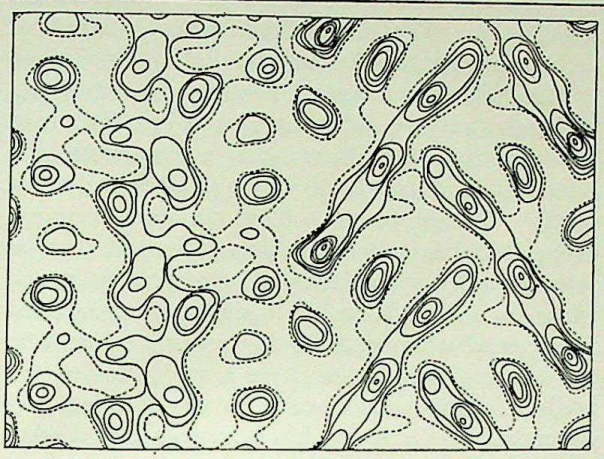

FIGURE 19 – *Section through several myoglobin molecules. The long ridges of high density on the right represent polypeptide chains* [10].

haemoglobin, myoglobin combines reversibly with molecular oxygen; it is used by vertebrates to store oxygen in muscle.

Kendrew's model is the result of a three-dimensional X-ray analysis which shows the distribution of density in the myoglobin molecule along a series of parallel sections, rather like a series of microtome sections of a tissue, but on a thousand times smaller scale (figure 11) [10]. Figure 19 is an example of one such section which takes the form of a contour map with the hills and valleys signifying regions of high and low density. Usually sections of this kind, obtained by X-ray analysis, can resolve individual atoms, like the ones of penicillin shown in figure 12, but when analysing molecules as large as proteins, it is convenient to approach atomic resolution by stages. At this first stage of the three-dimensional analysis of myoglobin the resolution is only 6 Å, which is too coarse to resolve individual amino-acid side-chains, let alone atoms, but sufficient to show the haem group and large parts of the polypeptide chain. The chain, at this resolution, is a cloud of high density, like the vapour trail of an aeroplane, without much differentiation; the haem group, with its central iron atom recognizable by its high density, resembles a slightly squashed sphere. By tracing the course of the regions of high density through the different sections, Kendrew was able to build a plausible model of the myoglobin molecule (figure 13). This reveals a remarkably intricate structure, much more complex than anything we had imagined. The polypeptide chain is folded and turned in a complicated three-dimensional basketwork and enmeshes the haem group in a subtle manner. From one of the density sections it is possible to infer the point of attachment of the iron atom to the polypeptide chain, but in addition there seem to be at least two other points of contact between the chain and the protoporphyrin ring. Only the side on which the oxygen attaches itself is left clear. The nature of the chemical groups involved in the interaction between the haem group and the polypeptide chain cannot yet be resolved, and neither can the configuration of the chain itself, but from the distance between different portions of the chain in regions where it folds back on itself, and from its cylindrical shape, one would guess that it is coiled into a helix. What kind of helix is not yet clear.

Kendrew, at the next stage of his analysis, will try to raise the resolution to 2 Å, which should suffice to show up the turns of the helix and the positions of the larger side chains. Atomic resolution is still far off and fraught with difficulties.

The writer's own work is concerned with the structure of haemoglobin, which has a molecular weight of 67 000, four times larger than myoglobin, and contains four polypeptide chains and four haem groups. It consists of two identical halves (not four identical quarters) which are joined together to form a roughly spheroidal molecule with the dimensions $55 \times 55 \times 70$ Å. While the model of myoglobin is based on an analysis of the density distribution along a series of closely

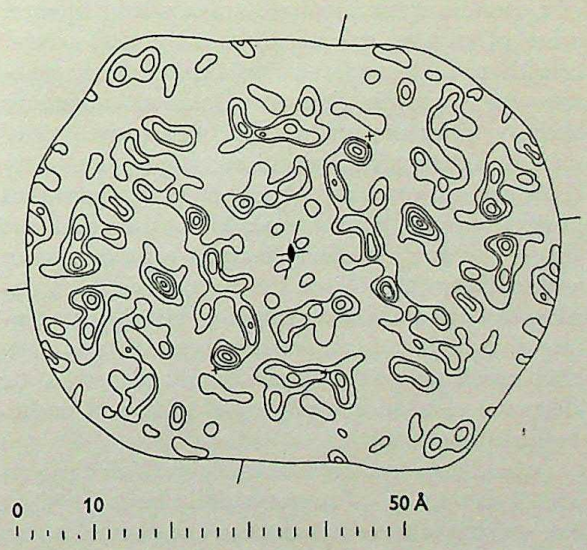

FIGURE 20 – *Picture of the density distribution in the haemoglobin molecule, seen in projection on a plane. The symbol at the centre represents an axis of two-fold symmetry which requires that the molecule consists of two structurally identical halves. Peaks represent regions of high density. Contours in the valleys are omitted.*

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spaced sections through the molecule, all we can show of haemoglobin at present is the density distribution seen in projection on a plane (figure 20). This figure has a resolution of 2·8 Å, in principle sufficient to distinguish significant detail, but suffers from the overlapping of the many features that are all projected on the same plane, rather like the X-ray picture of an animal in which all the different organs appear superimposed. This projection, however, is merely a stage in the development of the three-dimensional analysis, which should lead to a molecular model in the not too distant future. We hope that this will show the arrangement of the four haem groups and of two out of the six sulphydryl groups, and the general layout of the four polypeptide chains. With luck we might also be able to see how the two halves of the molecule are joined together and the relation between the structures of myoglobin and haemoglobin [11].

The reader may well ask what purpose is served by these elaborate analyses. Supposing a good fairy appeared in the night and left us a folder giving the positions of all 10 000 atoms in the haemoglobin molecule, what good would it do us? Sanger's analysis of insulin has taught us what kind of primary structure to expect in proteins, but before Kendrew's myoglobin model nothing was definitely known about their secondary or tertiary structure. The complete solution of a protein structure by X-rays would open a mine of stereochemical information and would furnish rational explanations for the basic phenomena of protein chemistry, such as enzymatic activity, denaturation, solubility, accessibility of chemical groups, and species specificity. Probably some architectural principles applying to proteins generally would also emerge, but at this stage one cannot be sure if such principles exist or if they would be overshadowed by the specific individuality of each particular protein. Detailed understanding of protein structure is crucial for biology and medicine and may ultimately lead to a far deeper understanding of physiological and pathological functions.

In addition to these wider aims, which apply to the X-ray study of any crystalline protein, there are specific reasons for concentrating on the structure of oxygen carriers. The attraction is the unique and central position which iron-porphyrin complexes take up in cellular and animal metabolism. Haem by itself does not form a compound with molecular oxygen; this property is conferred upon it by the combination with the specific protein globin. We want to determine the nature of the bonds that hold haem and globin together.

The difference between the oxygen-combining properties of myoglobin and haemoglobin needs consideration. The former has a hyperbolic dissociation curve, while the latter exhibits a characteristic sigmoid curve. This happens because myoglobin has only one haem group, so that its reaction with oxygen follows a unimolecular law, while in haemoglobin the four haem groups influence each other's affinity for oxygen. Moreover, the haemoglobin molecule as a whole undergoes a distinct structural change on oxygenation and reduction, while myoglobin does not. Knowledge of the positions of the four haem groups, and of any possible change in those positions during oxygenation and reduction, can be obtained only by X-ray analysis. It would perhaps help us to understand the two most important physiological properties of haemoglobin: the sigmoid oxygen dissociation curve and the Bohr effect, that is the fact that oxyhaemoglobin is a stronger acid than reduced haemoglobin, and therefore absorbs less carbon dioxide. The structural differences between adult and foetal haemoglobin would be another important problem to solve.

That the iron-combining property is specific to the combination of haem with globin has already been stressed. Other, quite different, chemical reactivities are conferred upon the same haem group if it is combined with different proteins. In haemoglobin the iron atoms remain in the ferrous state throughout oxygenation and reduction. In the cytochromes which act as respiratory enzymes, catalysis involves rapid oxidation and reduction between the ferrous and ferric states. The peroxidases and catalases, by contrast, are normally in the ferric state and serve as catalysts for the oxidation of certain reducing agents by peroxides and some other strong oxidizing compounds. The molecular weight of catalase (224 000) is too large for crystallographic study, but peroxidase (41 000) and cytochrome C (13 000) lie within the possible range. Cytochrome C has been crystallized for the first time and examined by X-rays in a preliminary way by G. Bodo [12].

At present oxygen carriers and respiratory enzymes in which the site of the catalytic activity is known probably offer a more promising field for X-ray analysis than, say, proteolytic enzymes which contain no prosthetic group. It may, however, become possible to learn something about the catalytic sites of other enzymes by studying crystalline complexes of enzyme and inhibitor.

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So far this account of protein structure has been restricted almost exclusively to soluble proteins, and little has been said about the fibrous ones whose main function is contractility, protection, and structural support. Pauling and Corey's α-helix and pleated sheet, and the triple helix recently proposed by several groups of workers for collagen, have established the general principles on which fibrous proteins are built. A major unsolved problem left now is the detailed structure of the two muscle proteins, myosin and actin, and its relation to the sliding mechanism of muscular contraction recently discovered by H. E. Huxley and A. Hanson, and by A. F. Huxley and F. Niedergerke. This is a large problem of its own which belongs to quite a separate field of molecular biology, the field of structure and movement, which has been discussed by H. E. Huxley in a recent article in ENDEAVOUR [13].

PROTEIN STRUCTURE, MUTATIONS, AND SPECIES SPECIFICITY: STRUCTURE OF ANTIBODIES

Having discussed the structure of nucleic acids and proteins, we might consider once more the relation between a gene and its product by examining the effect of genetic mutations on proteins. If there really exists a direct correspondence between the sequence of base pairs in DNA and the sequence of amino-acid residues in a polypeptide chain, then a mutation, consisting in a change of the genetic code, should express itself in a change of amino-acid sequence.

Let us begin by considering the effect of a single mutation. Sickle cell anaemia and haemoglobin C disease are two congenital defects which are inherited in a Mendelian manner. Each is due to the appearance of an abnormal haemoglobin which harmfully alters the properties of the red cell. During the last year V. M. Ingram and J. A. Hunt have discovered that the abnormality consists in the change of a single one out of about 300 amino-acid residues in the haemoglobin half-molecule, one particular glutamic acid residue being replaced by valine in sickle cell haemoglobin and by lysine in haemoglobin C (figure 21) [14]. Looked at from the point of view of protein chemistry, this discovery shows us that the alteration of a single residue sensitively placed in a protein molecule may have a profound effect on its physiological properties. Genetically, the fact that the change is the smallest possible suggests that it is due to a single mutation in the haemoglobin gene, and the observation that it is the same residue which is replaced in the two different abnormalities suggests that, on the molecular scale, the position of the sickle cell mutation is very closely linked, if not identical, with the position of the haemoglobin C mutation.

Hb A . . . $\overset{+}{\text{His}}$—Val—Leu—Leu—Thr—Pro—*Glu*—$\overset{-}{\text{Glu}}$—$\overset{+}{\text{Lys}}$ . . .

Hb S . . . $\overset{+}{\text{His}}$—Val—Leu—Leu—Thr—Pro—*Val*—$\overset{-}{\text{Glu}}$—$\overset{+}{\text{Lys}}$ . . .

Hb C . . . $\overset{+}{\text{His}}$—Val—Leu—Leu—Thr—Pro—*Lys*—$\overset{-}{\text{Glu}}$—$\overset{+}{\text{Lys}}$ . . .

FIGURE 21 – *Sequence of amino-acid residues in a small segment of one of the polypeptide chains in haemoglobin. The three rows show the sequence in normal human haemoglobin (Hb A), in sickle cell anaemia haemoglobin (Hb S), and in haemoglobin C disease (Hb C). It is seen that all the residues except one (in italics) remain unaltered.*

Ingram's discovery has demonstrated most clearly and strikingly that mutations work on a very fine scale, but the same is really evident from the way evolutionary changes manifest themselves in the structure of proteins. It is well known that the amino-acid composition, crystalline form, antigenic character, and other properties of a given protein vary from species to species. Recent work by F. Sanger, V. Du Vigneaud, C. H. Li, and others has given us an insight into the alterations in amino-acid sequence which such differences between the species imply. Sanger and his colleagues found, for instance, that the great bulk of the insulin molecule remains unaltered in species as widely separate as cattle, horse, pig, sheep and whale. Replacements occur in only one 'species-sensitive' region which lies between the two halves of the cystine residue forming the internal bridge in one of the two chains. One alanine is liable to be exchanged for a threonine, one glycine for a serine, and one valine for an *iso*leucine [15]. These changes are evidently harmless, though the cystine bridge, to which the variable residues are attached, is known to be essential for hormone activity. It is not clear whether these particular three residues are variable because they correspond to an unstable region of the insulin gene or because mutations leading to the replacement of any of the other 48 residues in the insulin molecule would be lethal. Species differences consisting in the replacement of one or two residues in a long chain have also been found in other hormones.

The variations in these peptides and proteins give expression to some of the mutations by which the species have evolved. What impresses us most about them is that mutations appear to work on so fine a scale as to affect only one residue at a time. Many of us believe, though we would find

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it hard to justify our belief, that the cause of such mutations is the replacement of a single base pair in the double helix of DNA which constitutes the gene. Even if this belief cannot be verified yet, it should at least be possible to test the sequence hypothesis indirectly by correlating genetic data on the sequence and spacing of mutating loci within a gene with chemical data on the variations in amino-acid sequence controlled by that gene. Benzer's bacteriophage system, in which he was able to map over a hundred loci within a single gene, offers a possible opening for such work.

One other problem which might be re-examined in this context is the structure of antibodies. The question concerns the difference between two antibody molecules made in response to two different haptens. Does it merely lie in a different folding of the polypeptide chain in two chemically identical proteins, or is it due to an alteration in amino acid sequence? The extremely sensitive methods of analysis now being developed by Sanger and by Ingram might furnish an answer to this question.

STRUCTURE OF SMALL VIRUSES AND MICROSOMAL PARTICLES

While bacteriophage has been the virus of choice for studies of genetics and growth, the small plant viruses have proved excellent material for research on molecular structure. Unlike bacteriophage and other large viruses, these contain only RNA and no DNA. The RNA is combined with proteins in proportions which vary widely in different viruses. In the nineteen-thirties, when the plant viruses were first crystallized, someone called them 'naked genes', because they seemed to be self-reproducing molecules. Today we know that this was a misnomer. The beautiful work of H. Fraenkel-Conrat in Berkeley and of G. Schramm and A. Gierer in Tübingen has shown that the RNA alone is infective and carries genetic information. Thus the RNA of the virus is the gene, and we know now that it is not naked, but well wrapped up in a protein coat.

X-ray studies of viruses, like those of smaller crystalline proteins, were begun by J. D. Bernal about twenty years ago. In view of the truly gigantic size of the molecules and the great technical and theoretical difficulties involved, the results so far achieved, especially with the mosaic virus of the tobacco plant, are most impressive. Tobacco mosaic virus (TMV) has a molecular weight of 45 millions and consists of 5 per cent RNA and 95 per cent protein. It has the shape of a rod with a length of 3000 Å and a width of 170 Å, when wet (figure 10). What is remarkable about this rod is its crystalline character. X-ray pictures show that it contains a molecular pattern repeating at regular intervals along its length, which implies that the protein part of the virus, at least, must consist of a regular array of identical sub-units.

In the course of the last five years a combination of independent X-ray studies by J. D. Watson, D. Caspar, Rosalind Franklin, and A. Klug, and electron microscope observations by R. Williams, R. Hart, and H. E. Huxley, have given us an intriguing picture of the internal structure of TMV (figure 15). Our first surprise was to discover that the rod is hollow, having a hole of 40 Å diameter running down its centre. Wound around this hole is a single long chain of RNA containing about 8000 nucleotides. We know nothing as yet about the geometry of this chain, but from the fact that it is at least ten times longer than the entire virus we can infer that it probably forms a helical coil, like the tungsten filament in an electric lamp. Enveloping the RNA coil is a helical array of protein molecules, as shown in figure 15. This much has been discovered by physical studies [16].

Chemical studies go further in telling us about the constitution of these protein molecules. They show that each has a molecular weight of 17 000 and probably consists of a single chain of 150 residues. The terminal residues at each end of the chain have already been determined, but the rest of the sequence is still unknown.

Several other rod-shaped plant viruses have been discovered with a structure closely related to TMV. The other small viruses look spherical under the electron microscope, and most of those that have been crystallized seem to be cubic, including poliomyelitis virus (figure 8). The structure of these spherical viruses is of great biochemical and medical interest. Their X-ray analysis shows how crystallographic symmetry can sometimes simplify a problem of apparently hopeless complexity, because in this particular instance the cubic symmetry of the crystals implies that the virus particles themselves consist of a certain number of symmetrically arranged sub-units of identical structure.

R. Markham suggested several years ago that turnip yellow virus consists of a shell of protein enclosing a core of RNA. His idea and the cubic symmetry led J. D. Watson and F. H. C. Crick to investigate how many different kinds of regular arrangements of circles can be drawn on the surface of a sphere. They soon found that this

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problem had already been solved by Plato. Omitting arrangements that are inapplicable to asymmetrical molecules, the only possible numbers of circles are 12, 24, and 60, each having a different symmetry recognizable by its X-ray diffraction pattern. When seen under the electron microscope at sufficient resolution, such particles should appear as regular polyhedra rather than spheres, and have a mulberry-like surface (figure 14).

Results found to date bear out these expectations. For instance, one small spherical plant virus with a total molecular weight of 9 millions seems to have a protein shell containing 2×60 identical protein sub-units of molecular weight 57 000 arranged on the faces of a regular polyhedron. The RNA in the core is folded in a manner still unknown [16]. A larger insect virus (which contains DNA) can be clearly recognized as an icosahedron under the electron microscope [17].

Present observations thus lead one to conclude that small viruses consist of a core of nucleic acid which carries the genetic information, protected by a shell which is made up of many symmetrically arranged protein molecules of comparatively small size. The nucleic acid core carries the code for its own replication and for the manufacture of its protective protein, as well as for any enzymes required in one or other of these processes and not supplied by the host.

The microsomal particles associated with the synthesis of protein in normal cells appear to resemble the spherical viruses in shape and composition. This suggests that a virus infection may be equivalent to the injection of an apparatus for the manufacture of the wrong protein.

CONCLUSION

During the past six years molecular biology has changed from a subject of speculation and uncertainty to an exact science. Powerful methods of X-ray analysis have led to the elucidation of the basic molecular patterns of the most important types of protein fibres and of deoxyribonucleic acid; they are now beginning to reveal the structure of globular proteins and small viruses.

There is hope that genetic events in viruses and micro-organisms may soon be understood in molecular terms. A start has also been made in elucidating the genetic control and molecular mechanism of protein synthesis. Muscular contraction is another phenomenon which may soon be understood in molecular detail.

In many fields, on the other hand, knowledge of structure is still rudimentary and shows no correlation with our knowledge of normal and pathological function. Cell permeability and the whole problem of active transport through membranes is an important example. If the rapid development of molecular biology continues, these gaps may gradually be filled, and more and more of the fundamental workings of living systems may become understood in terms of the interaction between molecules of known structure, to the great benefit of biology and medicine.

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Chemistry and alchemy in sixteenth-century Cracow

WŁODZIMIERZ HUBICKI

The history of early chemistry in Poland is very little known, but there was a period, the sixteenth century, when Cracow was one of the centres of alchemy and chemistry. This article surveys the state of these subjects at this time. It includes a description of an early instance of a claim to have produced gold by transmutation being put to the test of quantitative analysis.

Cracow, the capital of Poland in the middle ages, has one of the oldest of the Central European universities; it was founded in 1364. During the sixteenth century there probably were schools of the various branches of magic—Faust started his researches there [1]. These studies were, even if unsuccessfully, forbidden by the University, which was, however, a renowned centre for its officially recognized work in astrology and alchemy. The Cracovian astrological calendars had a European reputation until the seventeenth century, and the alchemical schools were of comparable repute.

It is easy to see why 'practical alchemy,' the precursor of chemistry as opposed to the alchemy of the philosopher's stone and the elixir, should have such importance in sixteenth-century Poland. The time was one of vigorous mining activity, of the establishment of the silver mines of the Tatra Mountains, new lead mines near Olkusz, and copper, brass, and lead foundries [2]. Cracow was the centre of this mining and metallurgy.

One of the most important foundries—a contemporary tells us it 'gave off smoke like Ætna'—was at Mogila, just outside Cracow. It was established by Jan Thurzo, and produced copper that was exported to Flanders, France, and Britain. The Royal Mint at Cracow used silver obtained from this copper ore for its coins, and in 1516 a royal decree established a laboratory.

This 'Camera Separatoria' was to study the separation of the precious metals and the manufacture of the fluxes and acids needed. It was directed by Kasper Ber, a Cracovian, as was Thurzo. Ber's reputation as a metallurgist was already sufficiently high for him to have students from Hungary, Germany and Bohemia.

Agricola [3] describes the Polish cupellation method, possibly devised by Ber, and also describes the Polish method for the extraction of lead [4].

That Agricola should describe the Polish techniques of extraction shows that they were fairly elaborate. The Poles were also surprisingly far advanced in assaying methods. A royal decree of 1555 laid down that pure tin, imported from Saxony or England, was to be marked with the king's initials and a crown. The purity, for these purposes, was measured by comparing the specific gravity of a sample with that of pure tin.

This method hardly suggests an exact science, but, in general, assaying methods were highly accurate. The balance was of striking importance even at this time, and many sixteenth-century assaying textbooks describe its construction. A typical book is that of C. Schreittmann [5]. He not only describes the balance, but also goes into detail about the weights, stating that they should be made of silver, kept in special boxes, and touched only with tweezers. It is a highly practical book, despite his referring to his smallest weights as atoms, as light as 'dust seen in the sunshine'.

Agricola, again, describes the care to be used in weighing [6]. 'It is necessary that the assayer who is testing ore or metals should be prepared and instructed in all things necessary in assaying, and that he should close the doors of the room in which the assay furnace stands, lest anyone coming at an inopportune moment might disturb his thoughts when they are intent on the work. It is also necessary for him to place his balances in a case, so that when he weighs the little buttons of metal the scales may not be agitated by a draught of air, for that is a hindrance to his work.'

His description [7] of the method of gold assay is interesting. 'One drachma [about 3·6 g] of examined sample was smelted together with 1 ounce [about 30 g] of lead with the addition of flux that is so-called artificial salt. The melted-out grain of gold was weighed. The next step was the determination of the quantity of silver in gold, which was done by means of touchstone and

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FIGURE 1 – *The Polish hearth for lead smelting* [4]. *V, Polish hearth; P, Westphalian method of melting; Q, heaps of charcoal; R, straw; T, crucibles.*

assaying needles, or by etching it with nitric acid. The silver ore was determined by means of cupellation with lead.' The accuracy of these techniques is very striking.

Some sample alloys are preserved, together with the analyses by one of Kasper Ber's students. I have shown that, in fact, these results differ by less than one per cent from results obtained nowadays. This kind of accuracy needed extremely pure reagents. Nitric acid containing chlorides, for example, would lead to the dissolution of the gold as well as hindering that of the silver. The acid was purified by distillation over silver filings.

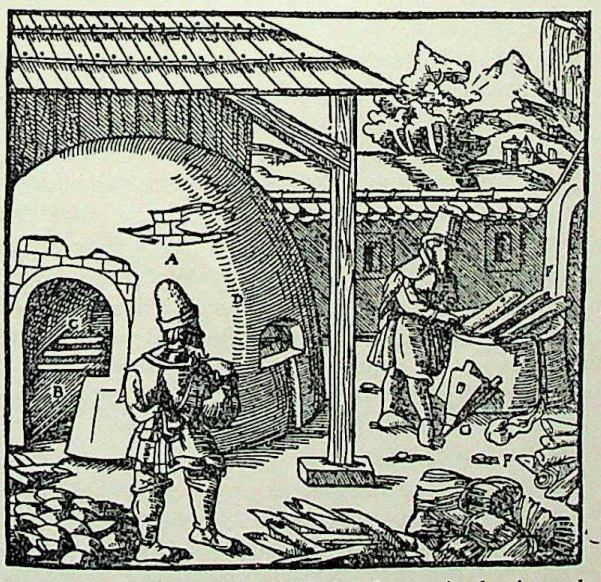

FIGURE 2 – *The Polish cupellation furnace in the sixteenth century* [3]. *A, furnace similar to an oven; B, passage; C, iron bars; D, hole through which the litharge is drawn out; E, crucible with dome removed; F, thick sticks; G, bellows.*

The representative, not to say originator, of another approach to chemistry arrived in Poland during this period. Paracelsus came to Poland about 1520, and had a medical practice in Cracow—he is recorded as curing the son of a rich Cracovian, Francis Boner. The prescription was *oleum philosophorum*, produced by distilling vegetable oil with powdered brick.

Little is known about Paracelsus's actual students. We know, however, that two of them, David Meyer and the court physician Albert Baza, were Poles. We have not much information about their activity; the Paracelsan group of the 1560s has left more traces.

Cracow was then one of the great centres of the movement—Paracelsus lived at Wroclaw [Breslau], immediately before his death, but there were lively commercial and intellectual contacts between the two cities.

The group at Cracow talked of alchemy, but not in the sense of transmutation. Thus Adam Schröter, from Nissa, writes: 'Only idiots think that alchemy is the knowledge of obtaining gold. The aim of alchemy is to look for new medicines.' It is Schröter, too, who gives a fascinating description of the famous Wieliczka salt mines in Paracelsan terms [8], where he states that the salt comes from the three '*principia*', salt, sulphur, and mercury. Salt, he says, has many uses, such as melting ores and gold, or preparing '*aurum potabile*', the elixir, adding that having mentioned this 'he must finish the wise words, for not everybody may know the sacred mysteries of salt.' He was less mysterious about the oil found in these mines. 'Petroleum, believe me, is very important, especially when it combines with salt. The good God gave our country petroleum. Poland, country of riches, you arouse admiration everywhere.'

It was also Schröter, backed financially by Albert Łaski, a rich Polish palatine, who enormously helped the spreading of the Paracelsan theories by translating his *Archidoxae* and *De praeparationibus* from the comparatively little known German into Latin.

Another distinguished Paracelsan, a pupil and follower of Copernicus living for some time at Cracow, was Joachim Rheticus. He wrote, in 1567, to Joachim Camerarius, a professor in Leipzig, 'I have lately had news of a new German medical school, followers of Theophrastus. I met and spoke with Theophrastus himself in 1532. He certainly was a great man and published splendid work. . . . I am interested in chemistry and

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astronomy, but it is by medicine that I live.' His interest in chemistry is shown by another letter to the French philosopher Peter Ramus, where he said: 'As I am especially interested in chemistry, I have investigated its foundations and written seven books on it' [9].

There were other followers of Paracelsus in Cracow. Mardocheus Nelle, a Jewish physician, wrote a treatise on 'cements' (fluxes is the modern term), which was a commentary on *De transmutationibus metallorum* of Paracelsus; others were Jan Miączynski, a professor of medicine in the University of Cracow, who wrote a paper on the production of sulphuric acid from vitriol, and another professor, Kasper Skarbimirski, who edited a treatise *Epistolae alchemicae*.

Oddly enough, the greatest of the Paracelsans of that time is now almost completely unknown. This was Alexander Suchten [10]. He was the son of a rich and influential citizen of the port of Gdansk [Danzig]. Suchten himself is mentioned in the *Album diligentarium* of the University of Cracow as 'Alexius Zuchta de Gedano alias etiam Suchten dictus Kaszuba Polonus' who lectured 1521–22. In 1539 he was a canon at Frombork [Frauenburg], at the same time as Copernicus. He then studied in Louvain and in Italy and by 1549 he was librarian to Otto Heinrich, one of the electors of the Palatinate. Here he collected alchemical books, probably became acquainted with the works of Paracelsus, and gained a large following of students. In 1554 he returned to Cracow, eventually to become physician to King Sigismund Augustus; he left Poland in 1563, and went to the court of Prince Albert of Prussia, at Königsberg, and in about 1570 he went to Strassburg, where he printed his first works on antimony.

Suchten was a prolific writer of poems, theological dissertations, and treatises on chemistry and medicine. Among his most important works are *Liber unus de secretis antimonii* and *De antimonio vulgari*. These, and other works, have been translated into English by Cable in 1670 and A. Waite in 1893. He styled himself 'Chymicus' and referred to those attempting transmutation as alchemists, though he was expert in many of the alchemical techniques of distillation and purification and in several of his experiments clearly hopes to be able to show transmutation. He gives [11] accounts of the testing of some 'alchemical silver' which turned out to be an alloy of antimony, and of some alchemical gold that had been considered genuine by a goldsmith, a view which Suchten himself thought, at one stage, he had confirmed.

This experiment is extremely interesting, because it is the first published account of an experimentally proved refutation of transmutation by use of a balance. He says [the quotation is from Cable's translation]: 'Now come I to ☉,[1] and to tell you what happened to me is a wonder; when I had shewed to my good Companion, who thought nothing else but that he had got a great prize, he would not believe it, but took it into his own hands and at length found the truth, and began to question his ☉, and he spake, although I have often times tried it yet will not I trust myself, but take this half ounce of Gold and try it as you please. Mr *Hans* the Goldsmith saith it is true Gold. So took I the ☉ and brought it to the Goldsmith, and asked him if it were Gold, he said it was, and he could work it for ☉; for to the Sight, Touchston and Hammer it was very good ☉. Nevertheless I took the ☉ and did put to it 2℥ of ☾ to granulate and divid it in *aq. fort.* the ☾ dissolved it self, the ☉ fell to the bottom; this proof was true; this *sol* powder I mixed with ♁ crude and cast it through *Regulus*; let it flow in a Crucible and cast *Nitre* upon it, and drew the ♁ from the sol, and drew it off with ♄, this trial I found it also true. This *Sol* driven off did I cast again through, with ♁ and ♳; then took I the *Regulus* and let them go away before the Goldsmiths Bellows, for I had none; this trial the *Sol* stood also, on which every Chymist may justly rejoice.

'Nevertheless seeing the *Luna* had deceived me, therefore could I not trust the *Sol*, and caused it to be beaten thin, and amalgamated it with my ☿ of ♁, did set it four weeks in a gentle warmth, and took notice that the Amalgam was not hard, but soft, which was grievous to me; nevertheless I let it stand four weeks, and found my Amalgam much moister than when I put it in; then did I put it into a Crucible over a small fire, that the Crucible did not fully glow, and my ☿ flew away incredible swiftly from the ☉ that I did not mark it, but thought that my ☿ was coagulated into ☉, but when I weighed my ☉, I found no more than half an Ounce and 2 Quintileins [2 drams], and thought certainly that the two Quintileins were pure Gold.

'These two Quintileins I proved farther with ☿ of ♁ in the same manner as at the first time; then after that I evaporated the *Mercury* from it, and

[1] At this time the names of the substances, their astronomical names, and the astrological symbols were used indifferently. In this extract the symbols are to be interpreted as follows: ☉, *Sol*, Sun, gold; ☾, *Luna*, Moon, silver; ♁, antimony; ♄, Saturn, lead; ♳, sulphur; ☿, mercury.

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found my two Quintileins again; then was I merry, and hoped that my Companion would communicate his Preparation to me and I had Golden Mountains in my head, and I brought a good Message to my Companion, but he himself was not merry, but spake evil of it. *Well*, said he, *I have had great labour and pains with this* ☉, *and more than I do say. . . . The Gold which remaineth over and above to thee is not come from the Regulus, but is a composition of the Natural* ☉; *for I could not coagulate the Regulus into* ☉ *if there be not good Gold with it; thus Sol hath remained in the Test, but the other not, I know not how to bring it farther; and now understand the cause well, that that cannot be that I hoped.*'

His perseverance in the testing is impressive; his discovery that the substance was an alloy of gold was the result. He himself explains how there came to be any gold in the alloy—it had been added, a common technique of the alchemists, who assumed that the gold would grow, or multiply.

There was an anti-Paracelsan school. In Germany it was under the leadership of Thomas Erastus, in Cracow under Andrzej Grutyński, a professor at the University. Grutyński wrote two treatises, *Medicus dogmaticus* and *Solus philosophicus*, against Paracelsus, and these and similar books, and the books of Paracelsus and his followers, and the earlier works of Arnold de Villanova and Ramon Lull, were all to be found in two bookshops in Cracow that specialized in chemistry and alchemy, namely those of Zacheus Kessner and Johannes Thenaud.

Poland's importance in sixteenth-century political manœuvring caused a lively movement of people between Cracow and the main European towns. Thus Albert Łaski came to England in 1583 as a personal guest of Queen Elizabeth, and took part in a disputation at Oxford, in which Giordano Bruno defended his own and Copernicus's theories. He went to Kenilworth and worked with the alchemists John Dee and Edward Kelley, until his money ran out and he left, to avoid imprisonment for debt.

Łaski left in England his debts; he took with him John Dee and Edward Kelley, and their families. John Dee has left a description, in his memoirs, of a séance at the palace of the king, Stefan Batory, at Niepolomice, near Cracow. He made many other acquaintances in Cracow, among them Hannibal Roselli, the king's confessor and a publisher of hermetic books, and the professors of Cracow.

There was thus an enormous interest in alchemy in Cracow, by kings (such as Sigismund Augustus, Stefan Batory, and Sigismund III) as well as bishops (e.g. Krasński and Padniewski), noblemen, and the rest of the citizens. There were charlatans and their dupes, but, steadily, there was the voice of reason. There were sermons against the swindlers; Grutyński [12] wrote: 'A chemist looks for the right heat to make gold but, from the nature of metals, we know he cannot make gold but only a substance looking like gold. There is an eternal difference between natural gold and these mixtures. It is stupid and silly to sweat in smoke, to waste time and money in this way.'

Suchten, too, attacks Lull, supposed to have made the gold from which Edward III made rose nobles. This legend bristles with anachronisms, but Suchten attacks the practicability of the preparation in his *Elegia de Nobel Raymundi moneta Anglicana* [13]:

'. . . Ex aliis aurum nunquam fecisse metallis
Creditur, aut pueros hoc docuisse suos.
. . . Lulle valete,
Nobile Raymundi vana moneta tua est.'

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Trapped radicals

H. P. BROIDA

The theory of chemical reactions was vigorously stimulated by the realization that the products are the result of complex intermediate reactions, often involving free radicals. The study of these is handicapped by their reactivity and the small concentrations in which they occur, and it was not until recently that low-temperature physics and techniques of structural investigation were combined to study free radicals fixed in inert solids. The techniques that are used and the results of the study of these trapped radicals are discussed here.

One of the great contributions to scientific thought in the last half-century has been the realization that chemical reactions occur in a series of very definite steps. Although Arrhenius had indicated the importance of sufficient energy being available for reactions to take place, it was not until the 1920s that the detailed manner in which chemical reactions occur began to be widely interpreted in terms of molecular structure. Developments in the kinetic method, coming simultaneously with advances in theoretical chemistry and physics, revealed the importance of the fine details of reactions. It was realized that even relatively simple reactions such as the formation of water from hydrogen and oxygen actually were achieved by a complex series of intermediate steps. The overall reaction is made up of a large number of elementary reactions quite often involving highly reactive intermediates which normally have very short lifetimes. Free radicals are one important class of these intermediates.

A free radical is a chemical fragment—either atomic or molecular—which has an unpaired electron spin. The reactivity of the radical is due to the unpaired electron and, except in very unusual cases, such as large aromatic radicals which are stabilized by resonance, the lifetime of these species is considerably less than one second. Because of their high reactivity the concentrations of free radicals are generally very small, except at very high temperatures.

The problem of studying the structure and the properties of free radicals and their reactions is made very difficult by this high reactivity, the short lifetime, and the small concentrations which can be obtained. Nevertheless considerable information has been acquired by a variety of methods [1]. Deductions made from kinetic observations of reactions undergone by radicals have been particularly fruitful. Light emitted and absorbed by radicals, particularly in flames and hot gases, also has given considerable information [2]. Under some conditions it has been found possible to withdraw radicals from a chemical reaction into a mass spectrometer [3].

In recent years a number of workers more or less simultaneously decided to investigate the possibility, envisaged by G. N. Lewis [4] in 1941, of preserving radicals of low molecular weight by surrounding them with a rigid environment of molecules or atoms with which reactions could not occur. The results of these studies have demonstrated that highly reactive radicals can be trapped in solids at low temperatures [5] in concentrations near 1 per cent.

HISTORICAL ASPECTS

Radicals were first trapped inadvertently by many investigators who were interested in other problems. Three rough subdivisions of the historical background can be made: the first, going back to before the turn of the century, saw the development of very low temperature techniques and their application to observations on solidified gases; the second period started in the 1920s with the studies of reactions of radicals on cold surfaces; and the third phase began early in the 1950s with the application of a number of techniques of modern technology.

The first observations involving trapped radicals were probably made by J. Dewar in London, who reported various bright flashes and beautiful colours in products formed by the ultra-violet irradiation of a number of solids at liquid air temperatures [6]. R. J. Strutt [7] in 1911 unsuccessfully tried to trap at the temperature of liquid air the active species in the afterglow of a discharge through nitrogen. Between 1924 and 1935 L. Vegard, working at Leiden and at Oslo, made extensive investigations of the spectra emitted by condensed gases at temperatures down to 4·2° K when bombarded by electrons and ions [8]. His interest centred mainly on postulated explanations,

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including free radical reactions, of astronomical phenomena such as the aurora borealis. Similar studies were independently done by J. C. McLennan in Toronto [9].

The second stage is concerned with the development of chain mechanisms involving free radicals and with the effects of cold surfaces on these mechanisms. M. Bodenstein and F. Haber were active in this field. Later they were joined by Geib and Harteck, who became interested in the formation of hydrogen peroxide at low temperature. This work, involving the condensation of products of electrical discharges through water vapour, hydrogen peroxide vapour, hydrogen mixed with oxygen, and ammonia occupied the attention of others, including O. Oldenberg, W. H. Rodebush, Poljakow, G. I. Lavin, and J. R. Bates. In more recent years this phase has merged with the investigations of P. A. Giguère and of C. A. Winkler [10], who used discharge products, and of Everett and G. J. Minkoff, who have rapidly chilled explosion products.

The third and current phase was initiated by the work of F. O. Rice, who was probably the first to start with the aim of retaining highly reactive free radicals in solids and to show definite evidence of unstable chemical species being trapped on cold surfaces as a result of rapid cooling and condensation of decomposed parent compounds [11]. The development of physical methods of detection of free radicals—such as electron spin resonance, magnetic susceptibility, optical spectroscopy, and mass spectroscopy—has greatly accelerated the interest in studying solids in which radicals are trapped.

EXPERIMENTAL TECHNIQUE

It will be convenient to illustrate the types of phenomena which have attracted so much attention by using as examples some of the more vivid experiments. Discharge products of nitrogen when condensed at very low temperatures (below 25° K) give rise to some spectacular features. Moreover, these experiments, while simple enough to be easily reproduced, are indicative of the techniques and problems encountered in the studies of trapped radicals. For these reasons the method will be described in some detail.

Nitrogen gas at a pressure near 1 mm Hg passes through an electrodeless discharge (125 watt, 2450 Mc/s). The discharge products, mainly nitrogen atoms and molecules, are pumped in less than 0·1 sec to a Pyrex surface kept near 4·2° K by liquid helium (figure 6).

Discharge products of nitrogen have been studied for many years [12] and are known as 'active' nitrogen. In a darkened room this 'active' gas can be seen as a yellow glow filling the tube downstream from the discharge (figure 5). When the 'active' nitrogen impinges on the cold surface a bright, warm green glow immediately appears (figure 7). When the 'active' nitrogen stream is removed, a green glow (afterglow) persists on the cold solid for several minutes (figure 8). Either during deposition or after this glow has disappeared, a white solid (figure 9) can be seen with reflected light. If the solid is warmed to about 10° K the solid again begins to glow (warm-up glow), emitting green light (figure 14). Further warming to about 30° K causes a blue glow (figure 17). If argon is present, the deposited solid often peels away from the Pyrex surface (figure 18) after the glow has stopped.

The addition of small amounts of oxygen before the discharge causes the green glow to become more yellow (figure 11). This effect is more pronounced at higher temperatures. If the concentration of oxygen is greater than a few per cent, the glow of the solid is extinguished. When an excess of argon is added to the nitrogen, the glow from the solid during deposition is blue (figure 12). At high gas-flow rates a weak glow is sometimes observed in the gas near the solid (figure 19).

While the deposition proceeds, vivid blue and yellow flashes also occur on the solid. Colour photographs taken at 16 frames per second indicate that the flashes develop locally. On some occasions small spots of intense reaction are observed to travel fairly rapidly through the solid (figure 20). These regions are fairly reproducible. The reproducibility probably is due in part to temperature gradients and in part to imperfections in the solid. At times the heat liberated is so great that all the reactive products are destroyed and the light given off is enough to over-expose completely a single frame of the film.

To consider the light emission in more detail, it is useful to see the spectra obtained under various conditions. Figures 2–4 present spectra obtained during deposition of discharged nitrogen. The brightest features are the α (green) and the β (yellow) systems, which are shown in more detail in figure 3. The strong α group, consisting of a close set of five sharp lines near 5230 Å, is almost certainly emitted by nitrogen atoms. This transition is from the ^2D electronic state to the ground ^4S state. The weaker α′ and very much weaker α″ groups (figure 4*a*) occur when the α

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group is strong and may be associated with a loosely bonded complex of N and N_2. The β group, consisting of three diffuse lines near 5575 Å, probably is the ^1S–^1D transition of atomic oxygen. Between 3572 and 6390 Å the sharp-headed A bands are observed (figures 2*b*, 4*b*). The broad B bands (figures 2*c*, 4*c*) are of unknown origin, although it is possible that they are associated with NO_2. With excess argon present the blue emission (figure 12) is caused by a series of sharp bands starting at 4450 Å and extending into the ultraviolet (figure 2*d*). These bands are known as the Vegard-Kaplan system and are the $A^3\Sigma$ to $X^1\Sigma$ transition of molecular nitrogen. These bands also are emitted during the blue warm-up (figure 17) when excess argon is present. When there is less than 20 per cent nitrogen in the argon, an unidentified molecular band system appears relatively strongly (M bands in figures 2*d* and 4*d*). Argon also causes a shift in position of the β lines and a large change in the relative intensities of the α lines (figure 3).

When the nitrogen deposition ends and the solid begins to warm up there are large changes in the spectrum, as shown in figure 21. The first few frames after termination of the discharge (afterglow, figure 14) show the α lines of atomic nitrogen to be diminishing in intensity (frames 1–6, figure 21). However, as the liquid helium disappears (frame 6), the intensity of the α group increases, together with the β lines of atomic oxygen, giving the green warm-up glow (figure 15). At temperatures above 25° K the B bands begin to emit, causing the blue warm-up glow (figure 16). Above 30° K the emission is greatly diminished (frame 18, figure 21).

During the warm-up of the discharged nitrogen products condensed at 4·2° K considerable amounts of heat are evolved. If it is assumed that this heat release is caused by the recombination of nitrogen atoms, the number of nitrogen atoms trapped in the solid can be measured. Thus far, only unusual and not very precise calorimetric techniques have been employed because of the difficulties imposed by the procedure of trapping gas discharge products. Good thermal contact with the 4·2° K reservoir during deposition is essential to avoid losing the active species at higher temperatures. In addition, during warm-up of the solid deposit, the calorimeter needs to be thermally isolated. Moreover, a relatively large opening into the calorimeter is needed for bringing in the 'active' nitrogen without destroying the active species. These requirements are not easily met, and a compromise has to be made. One calorimeter rapidly warms the solid with electrical heating and compares the time for the solid discharged nitrogen to reach a fixed 'high' temperature with the time for the same mass of undischarged nitrogen. Typical curves for such times of warming are shown in figure 1. The discharged nitrogen warms up more rapidly and leads to an atom concentration of a few per cent [13].

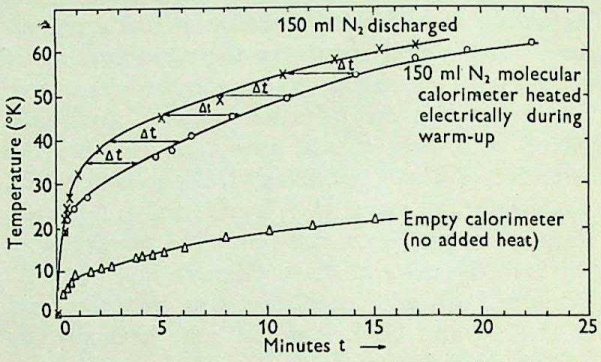

FIGURE 1 – *Warm-up curves for discharged and molecular nitrogen (heated electrically) and empty calorimeter.*

PRESENT KNOWLEDGE OF TRAPPED RADICALS

It is important to emphasize that the subject of trapped radicals is a new one and in a state of rapid development. Undoubtedly, therefore, in the next few years there will be considerable modification of the details of the brief review included here. However, the major conclusions should remain: (1) free radicals, made in a variety of ways, can be trapped in solids at low temperature; (2) these radicals can be preserved for indefinitely long times at low enough temperatures; and (3) heat, light, and chemical reactions can be obtained at low temperatures from these trapped radicals.

Good evidence has been reported for the trapping of three atomic free radicals: H, N and O. Electron spin resonance presents the most unequivocal evidence for the presence of unpaired electrons and thus proves that both H [14] and N [15] can be maintained in low-temperature solids. Hydrogen atoms, obtained from a discharge through hydrogen gas before deposition, have been observed with estimated concentrations near 0·1 per cent in solid H_2 at 4·2° K. Various solid acids at 77° K have been irradiated with γ-rays from cobalt 60 and show that H atoms are trapped; also the concentrations of atoms are unchanged after several months' storage at 77° K. Optical emission spectroscopy has indicated the presence of trapped N and O [16]. Chemical

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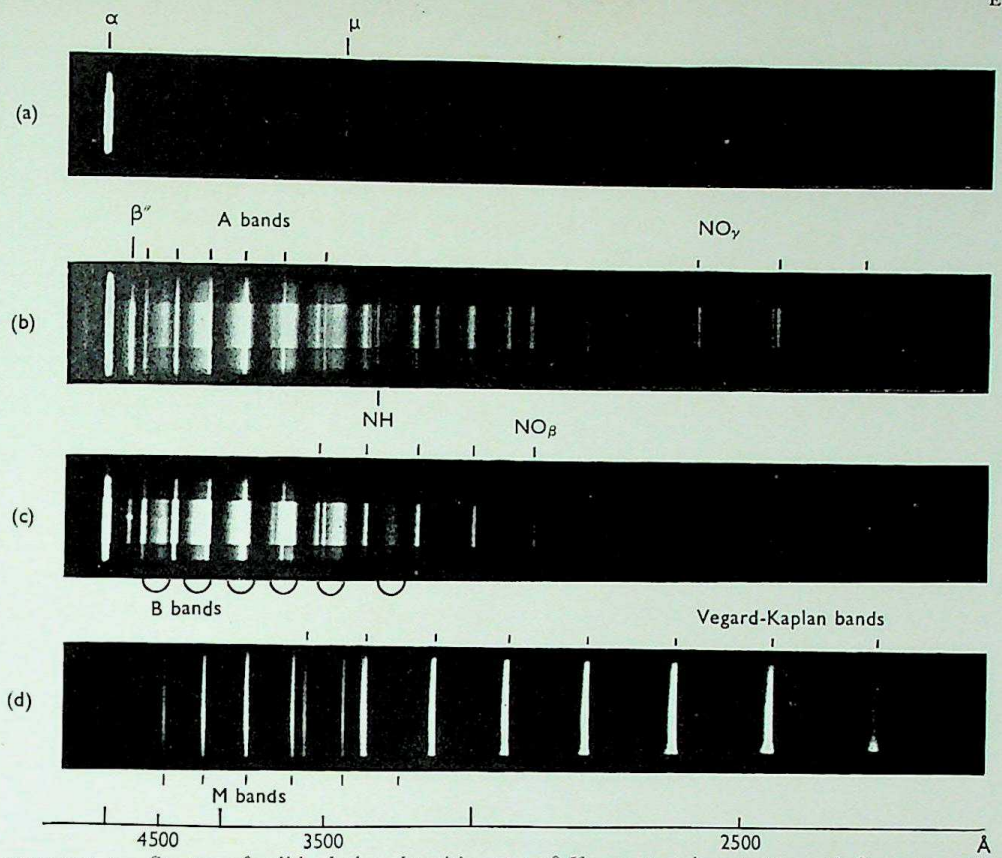

FIGURE 2 – *Spectra of solids during deposition at 4·2° K; quartz prism spectrograph f/4; 102 a-O plates.* (a) *Pure nitrogen; gas flow rate 1·5 cm³/sec; exposure times 1 min. and 3 min.* (b) *Commercial nitrogen; gas flow rate 1·5 cm³/sec; exposure times 1 min and 3 min.* (c) *Pure nitrogen with 0·1% oxygen added; gas flow rate 1·5 cm³/sec; exposure times 1 min. and 3 min.* (d) *5% nitrogen–95% argon mixture; gas flow rate 2 cm³/sec; exposure time 3 min.*

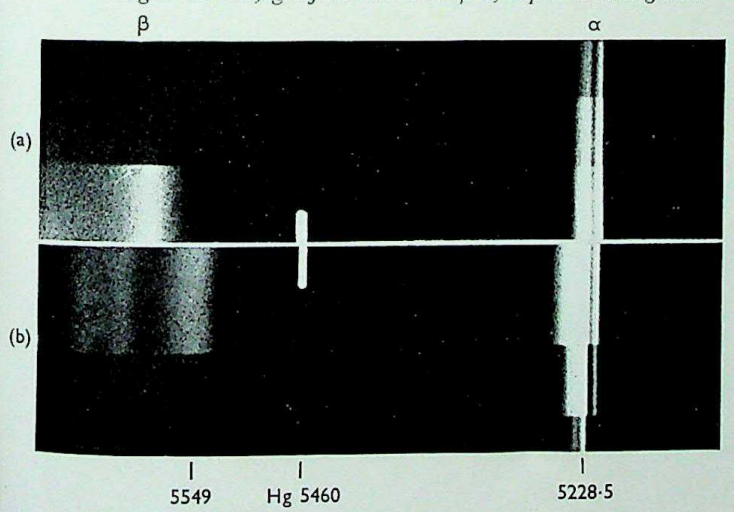

FIGURE 3 – *Spectra of solids during deposition at 4·2° K; glass prism spectrograph f/10; 103 a-F plate.* (a) *5% nitrogen–95% argon mixture; gas flow rate 2 cm³/sec; exposure times 7, 14, 23 min.* (b) *Pure nitrogen with 0·1% oxygen added; gas flow rate 1·5 cm³/sec; exposure times 3, 9, 27 min.*

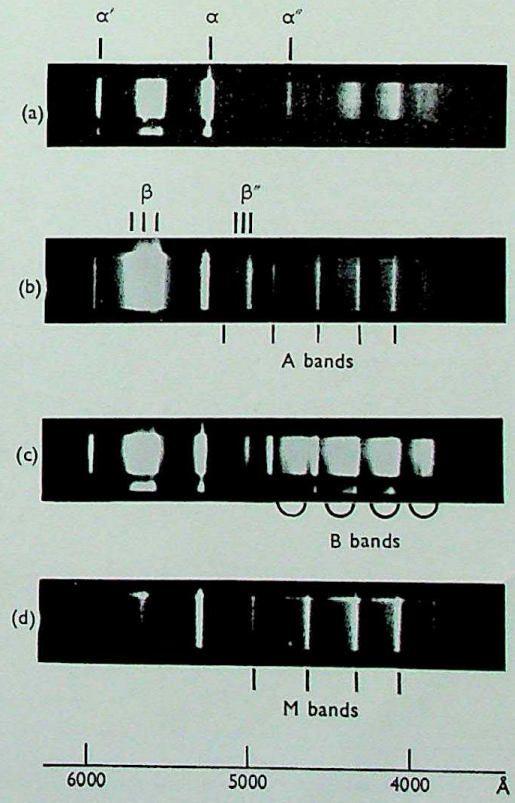

FIGURE 4 (*right*) – *Spectra of solids during deposition at 4·2° K; glass grating spectrograph f/0·625; 103 a-F film.* (a) *Pure nitrogen; gas flow rate 1·5 cm³/sec; exposure time 80 sec.* (b) *Commercial nitrogen; gas flow rate 1·8 cm³/sec; exposure time 5 sec.* (c) *Pure nitrogen with 0·1% oxygen added; gas flow rate 1·5 cm³/sec; exposure time 80 sec.* (d) *5% nitrogen–95% argon mixture; gas flow rate 2 cm³/sec; exposure time 5 sec.*

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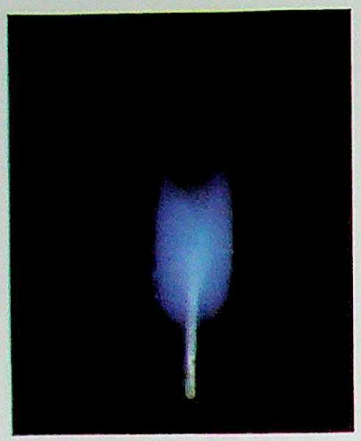


FIGURE 5 – *Gas afterglow from discharge through nitrogen with a small impurity of oxygen.*

FIGURE 6 (*right*) – *Pyrex surface upon which discharge products are condensed. Liquid helium is beginning to fill the chamber.*

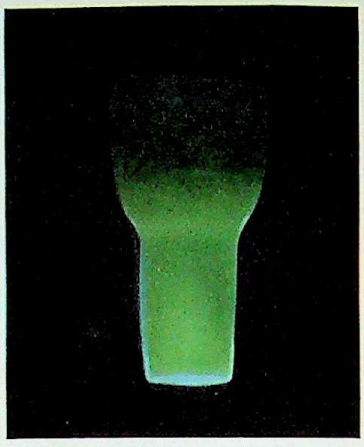


FIGURE 7 – *Nitrogen, discharge on.*

FIGURES 7–20 – *Colour photographs*[1] *of glows produced from solids condensed from discharges through nitrogen gas mixtures. Exposure times are usually of one second.*

FIGURE 8 – *Nitrogen, no deposition, discharge off, afterglow.*

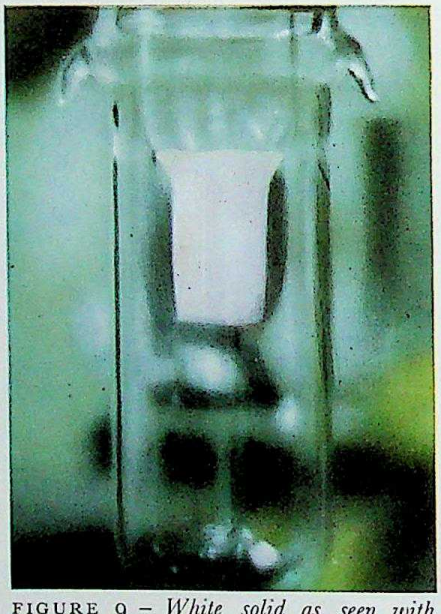


FIGURE 9 – *White solid as seen with reflected light.*

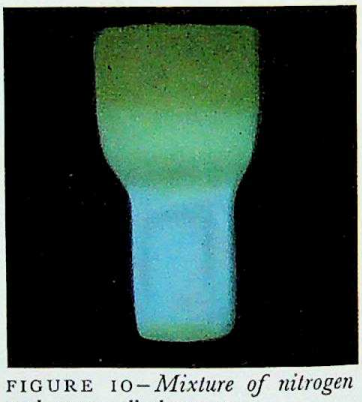
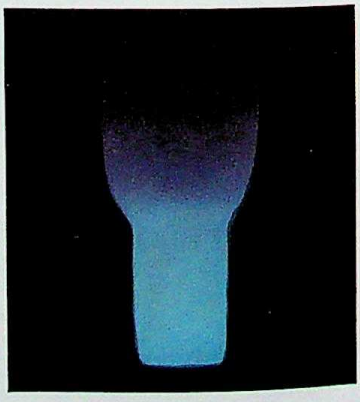


FIGURE 10 – *Mixture of nitrogen and argon, discharge on.*

FIGURE 11 (*left*) – *Nitrogen with small amount of oxygen, relatively high flow rate, discharge on.*

FIGURE 12 (*right*) – *Nitrogen with an excess of argon, discharge on.*



[1] *The colour photographs were made by Mr Warren P. Richardson, Photographic Services Section, National Bureau of Standards.*

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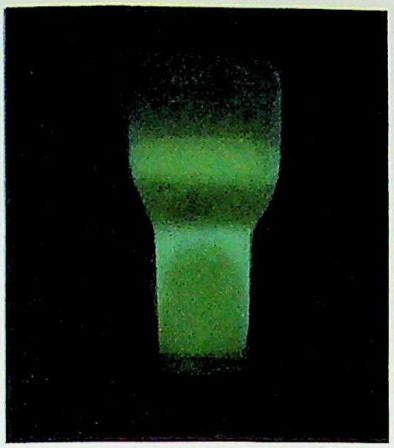

FIGURE 13

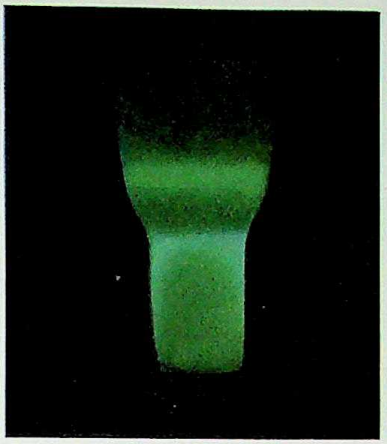

FIGURE 14

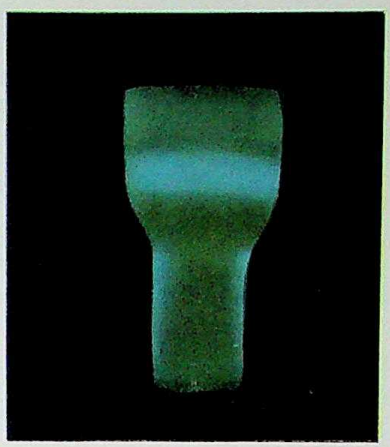

FIGURE 15

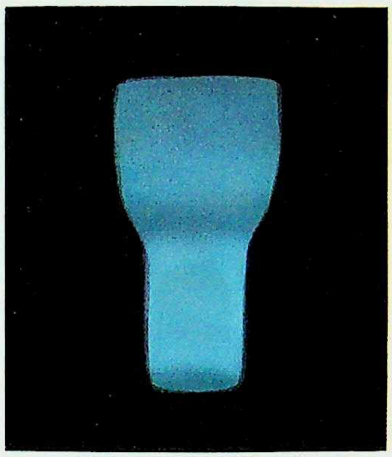

FIGURE 16

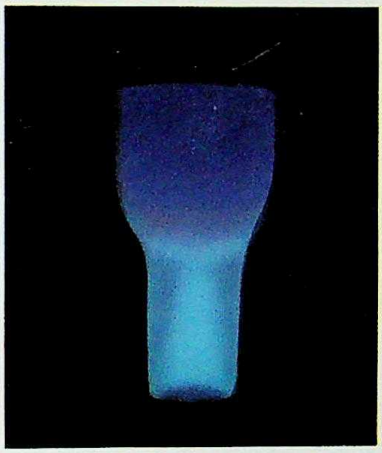

FIGURE 17

FIGURE 18 – *Solid as seen with reflected light shortly after figure* 17 *was obtained.*

FIGURES 13–18 – *Sequence of photographs taken immediately after figure* 10 *in approximately* 5*-second intervals during the warm-up of a mixture of nitrogen and argon. No gas flow.*

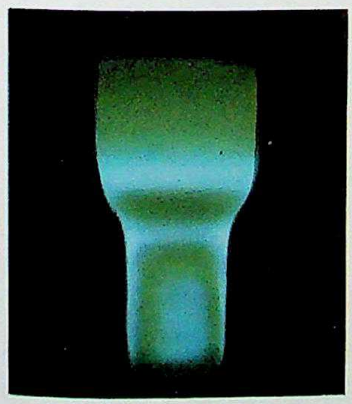

FIGURE 19 (*left*) – *Nitrogen and argon at high flow rate, discharge on.*

FIGURE 20 (*right*) – *Blue flash observed during deposition; exposure time of approximately* 1/25*th sec.*

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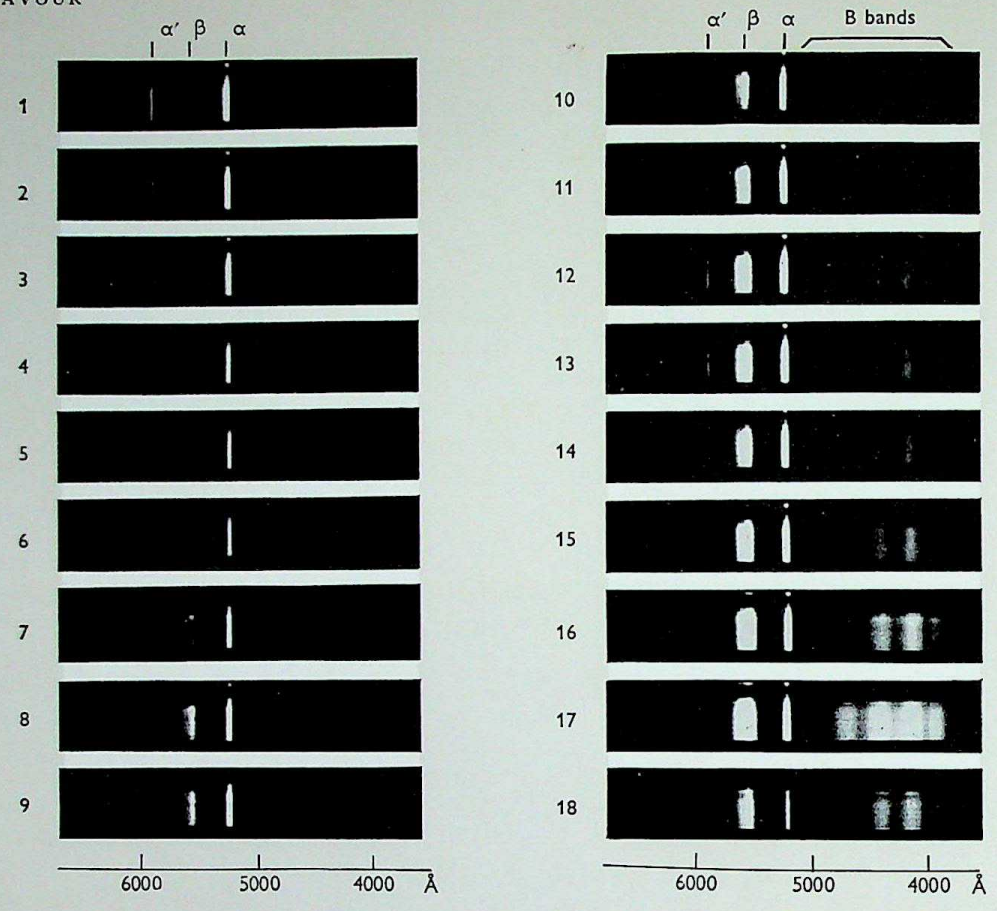

FIGURE 21 – *Spectra recorded during the afterglow and the warm-up of solid nitrogen containing* 40% *argon; glass grating spectrograph f*/0·625*;* 103 *a-F film; each frame is* 10 *sec exposure with* 5 *sec between consecutive frames. Frames* 1–6*, afterglow;* 7–16*, yellow-green warm-up period;* 17–18*, blue period.*

evidence [17], that is the interpretation of the products obtained on warming solids containing discharge products, supports the view that several per cent of these atomic radicals are trapped in solids below 20° K. Calorimetric experiments also indicate the presence of several per cent of both N and O [13].

The NH radical in solids has been the subject of many investigations. Rice and his co-workers [11] have studied extensively the products of hydrazoic acid decomposed by photolysis, pyrolysis, and electrical discharges. When such products are condensed on a surface cooled to 77° K a bright blue deposit is formed. This solid changes abruptly to white at 150° K, and sometimes the change in colour is accompanied by a violent explosion. Deductions from the compounds formed, supported by evidence from infra-red and ultra-violet absorption, indicate that NH radicals must have been present at some stage. The failure to find NH by mass spectrometry in the gas prior to deposition of the blue solid suggests that the concentration of NH in the solid is very small.

Many organic radicals have been trapped and detected both by optical spectroscopy and electron spin resonance. I. Norman and G. Porter [18] prepared a number of trapped radicals in 'glasses' of solid hydrocarbons at 77° K by photolysis with ultra-violet light and detected their presence by ultra-violet absorption spectroscopy. A large varity of organic radicals have been prepared by photolysis and detected by their free electron spins, using electron spin resonance. Usually the spectra are so complicated that specific identifications of the radical have not been possible. However, methyl, ethyl, and allyl radicals [19] have been positively identified as trapped in various solids.

PRESENT POSITION AND FUTURE PROSPECTS

During recent years the realization of the

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interesting potentialities of trapped radicals has led many investigators into this new field, and detailed attacks on the subject are now in progress. The broad plan involves, first, studies of methods of producing trapped radicals. Investigations are under way of the uses of pyrolysis, electric discharges, and chemical reactions to produce free radicals in the gas, followed by rapid cooling and condensation. Another approach is to use photolysis, γ-irradiation, or high-energy particle bombardment to produce the radicals directly in a cold solid.

Secondly, there are the problems associated with the detection and the measurement of the concentration of the trapped radicals. Electron spin resonance, optical spectroscopy (from 1400 Å to 25 μ), and calorimetry have been the most productive methods and are the methods most extensively being applied. However, instruments for mass spectroscopy, magnetic susceptibility, X-ray diffraction, electron diffraction, and dielectric measurements are being constructed and will soon be applied to this problem.

The third broad aspect of the field involves the problems of what takes place as the solid containing trapped radicals is brought to higher temperatures. Chemical methods are being developed for analysing the products, and the usual methods of chemical kinetics are being extended to lower temperatures to determine intermediate reactions. Information obtained by studying the relationships between the heat released and the temperature changes during warm-up is being analysed to determine the diffusion rates and the types of species originally present in the solid. Radiation found during the warm-up helps in the understanding of the mechanisms of the reactions.

Finally, we may consider developments associated with the trapping of the radicals. Some ideas are beginning to evolve concerning the chemical, statistical, and solid-state factors which control the various phenomena. It appears that the use of free radicals as tracers will increase our knowledge of the solid state, since a definite perturbation, the free radical, can be introduced into normal, molecular, solids. In addition, a new field of low-temperature chemistry may evolve from the study of the reactions of trapped radicals. This may lead to the formation of new chemical compounds or known compounds by new methods. Another interest centres around the possible use as a propellent of energy stored in the form of trapped radicals, because of the definitely higher specific thrust available from mixtures containing large concentrations of free radicals of low molecular weight.

Many problems must be solved before the various phenomena associated with the trapping of free radicals can be controlled. However, it is just this lack of understanding which brings interest and excitement to the field. At this time work in this field shows great promise.

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Satellite tracking: its methods and purpose

E. G. C. BURT

The Earth satellites contain instruments for transmitting information about the conditions of outer space. Such information is more useful if the satellites can be precisely located during their circuits, and, in addition, this plotting of the paths provides valuable data about the Earth. The methods used and the results obtained are discussed in this article.

The advent of the Earth satellite as a space laboratory has revealed new possibilities for many branches of research: investigators of solar radiations, geophysical phenomena, and cosmic rays, to mention but a few, are all contestants for the limited space available in the satellite laboratory, and all rely on the efficient working of the instruments contained within it. There is, however, much to be gained from a knowledge of the satellite's course through space, which can be derived from ground observations independently of the orbiting instruments. Apart from its intrinsic worth, information about the orbit assists the interpretation of other data: measurements of temperature and of cosmic ray intensity, for example, are much more valuable if they can be related to a particular time and place.

RADIO METHODS

Many satellite tracking systems exist, all of which are useful in certain circumstances. If the satellite carries a transmitter, various radio methods are available for establishing its whereabouts. These techniques are particularly useful during the early life of the space vehicle, when little is known about its path; the transmitter advertises its position over a wide area, so that it can be located without previous knowledge of the approximate orbit.

Probably the simplest arrangement is that which employs the Doppler effect. If the satellite transmitter sends out a continuous, unmodulated wave at a fixed frequency, the signal received on the ground exhibits a change in frequency, due to the relative velocity of satellite and observing station. The received and transmitted frequencies f and f_0 are related by

$$f = f_0(1 + \dot{s}/c)$$

where c is the velocity of light and s the distance between satellite and observer. If then the frequency is recorded as the satellite approaches and recedes, the radial velocity $\dot{s}$ can be calculated from

$$\dot{s} = c(f - f_0)/f_0$$

At the instant of closest approach the radial velocity will be zero, for then the relative velocity V_R is normal to the sight-line (figure 1); t seconds later the satellite will have moved a distance $V_R t$, so that

$$s^2 = s_0^2 + V_R^2 t^2$$

where s_0 is the minimum distance. Differentiating this equation with respect to time gives

$$\frac{t^2}{\dot{s}^2} = \frac{s_0^2}{V_R^4} + \frac{t^2}{V_R^2}$$

and a graph of $t^2/\dot{s}^2$ against t^2 should therefore yield a straight line, from which V_R and s_0 can be derived. A typical result, obtained early in the life of Sputnik 1 (15th October 1957), is shown in figure 2. The Doppler measurements were made by the Post Office Radio Station at Banbury.

The orbital period can be found by observing successive transits at the same station. However, the period is not simply the difference between the times of closest approach: a correction must be applied to allow for the fact that, in the intervening period, the rotation of the Earth has altered the observer's position relative to the satellite's orbit.

The accuracy with which the orbit can be determined from Doppler data alone depends, among other things, on the separation between the satellite and the observing stations, and between the stations themselves. For favourable triangulation these distances should be of the same order, but this cannot be achieved without the active co-operation of the satellite unless a large number of stations is in operation. Over the distances involved (normally a few hundred kilometres) the curvatures of the orbit and of the Earth are too large to be ignored—a fact which greatly complicates the analysis.

Refraction of the radio waves in the ionosphere can lead to considerable errors, particularly at the lower frequencies [1]. The 20 Mc/s and 40 Mc/s transmissions of the first two Sputniks, for example, frequently gave very different estimates of the

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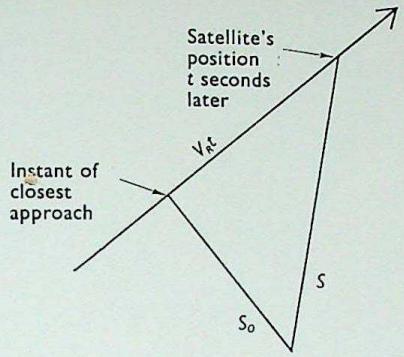

FIGURE 1 – *Relation between distance and relative velocity.*

satellite's position unless it happened to pass nearly overhead, in which case the electrical and optical paths are more nearly coincident. A much more satisfactory frequency—from the tracking standpoint—is the 108 Mc/s used in the American satellites; but of course the 20 Mc/s and 40 Mc/s transmissions of the Sputniks provide an excellent means of investigating ionospheric phenomena, particularly if the true path can be established independently.

A second tracking method which has been used successfully is the radio interferometer. Unlike the Doppler system, which measures radial velocity, the interferometer provides directional information, so that the two methods are complementary. As its name implies, the interferometer makes use of the interference pattern between the signals received at a pair of aerials, a pattern determined by the difference in the distance between the satellite and each of the receiving aerials. The separation between the aerials—say 50 to 100

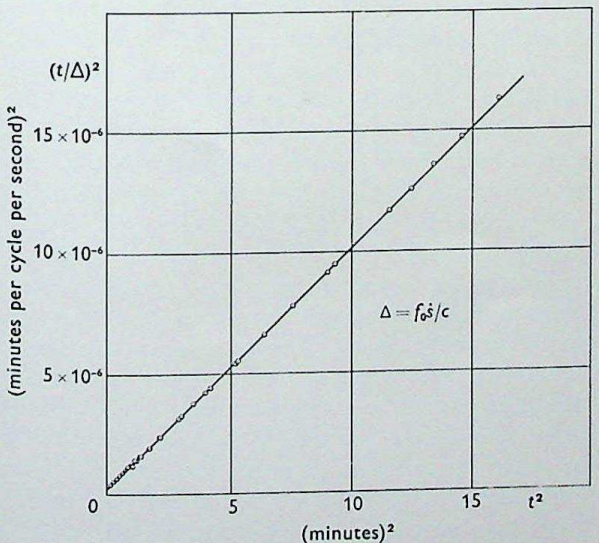

FIGURE 2 – *Derivation of V_R and s_0 from Doppler data.*

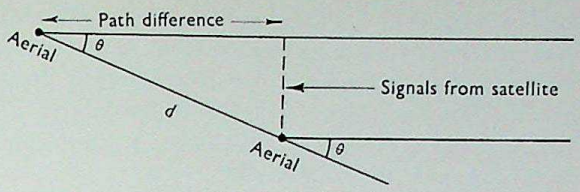

FIGURE 3 – *Principle of the interferometer.*

metres—is very small compared with the satellite's distance, so that the waves from the transmitter may be considered to traverse parallel paths (figure 3).

If the satellite happens to be at such an angle that the path difference is exactly equal to half a wavelength, i.e. if

$$d \cos \theta = \lambda/2$$

the combined output from the two aerials is zero, since the separate waves interfere completely. Evidently this will also occur whenever the path difference is equal to an odd number of half-wavelengths: as the satellite progresses along its orbit, the change in direction of the incoming radiation causes a succession of such zeros, the pattern depending on the direction of the track with respect to the line joining the aerials. During the interval between successive zeros the increase in $\cos \theta$ is λ/d, so that a record of the times at which the zeros occur gives immediately the rate of change of the direction cosine.

Instead of simply adding the signals at the two aerials it is preferable to measure their phase difference; in this way a continuous indication of $\cos \theta$ is available [2], and at the same time the measurement is almost independent of amplitude fluctuations.

Since the interferometer is insensitive to changes of a whole number of wavelengths in the path difference there are ambiguities in the measurement of the angle θ, which become more numerous as the aerial separation is increased. They can only be resolved from an approximate knowledge of the satellite's track obtained from another source, such as a second interferometer with a pair of aerials set much closer together, giving a 'coarse' and 'fine' indication. The 'fine' system obtained with the wider separation is of course more accurate, because a given electrical phase difference corresponds to a smaller increment in $\cos \theta$; but this cannot be carried too far, for the measurement of phase becomes more difficult as the separation is increased.

Another direction cosine can be derived from a similar set of aerials arranged at right angles to the first; it is then possible to define the direction of the line joining satellite and station during the

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satellite's transit. To deduce the orbit, additional information is necessary, such as the distance and relative velocity supplied by the Doppler data, or the directions from other interferometers at different places. Failing this, some assumptions about the orbit must be made.

Radio techniques such as these have been found to give very useful orbital information; indeed, the orbit of the first Sputnik was established with fair accuracy by radio methods only [3, 4], in spite of the inconveniently low frequency of 40 Mc/s, while radio interferometry is the standard method for tracking the American satellites, equipped as they are with 108 Mc/s transmitters.

Unfortunately the radio methods require active assistance from the satellite: if the transmitter ceases to function, so too do the Doppler and interferometer stations. The transmitter of Sputnik 1 failed some three weeks after launch, while that of Sputnik 2 lasted less than a week. The advent of solar batteries, which store radiation from the sun as electrical energy, has greatly extended the natural life of such equipment, but the risk of mechanical or electrical failure is still present, and it is prudent to seek other methods to allow for this contingency.

RADAR TRACKING

One possibility is to place the transmitter on the ground and to use the satellite merely as a reflector of energy—the radar technique. The distance is great and the reflecting area small, so that a sufficiently strong echo is obtained only if the radiated power is concentrated in a narrow beam—which means a large aerial system and a small field of view. Thus, unless the orbit is known tolerably well, it is no easy matter to ensure that the beam is directed to the right part of the sky at the right time. The large radio-telescope at Jodrell Bank [5], among others [6], has been used in this role; the method has the twin advantages of independence of satellite-borne equipment and of local weather conditions—a combination enjoyed by neither radio nor visual methods.

OPTICAL TECHNIQUES

Optical observations, however, have played the major role in establishing accurate orbits for those satellites which have been visible. Both Sputnik 3 and its carrier rocket, for example, are easily detected by the naked eye, while to those who knew its habits Sputnik 2 became a familiar object during its six months' sojourn in the heavens. Many methods are used, ranging from naked-eye and stop-watch observations to refined telescopic and photographic techniques. The former are invaluable for prediction purposes, since no lengthy process of data reduction is involved; in this way it is possible to forecast with fair accuracy the satellite's expected position at future transits—a service which is of great help in preparing for observations with the more accurate instruments.

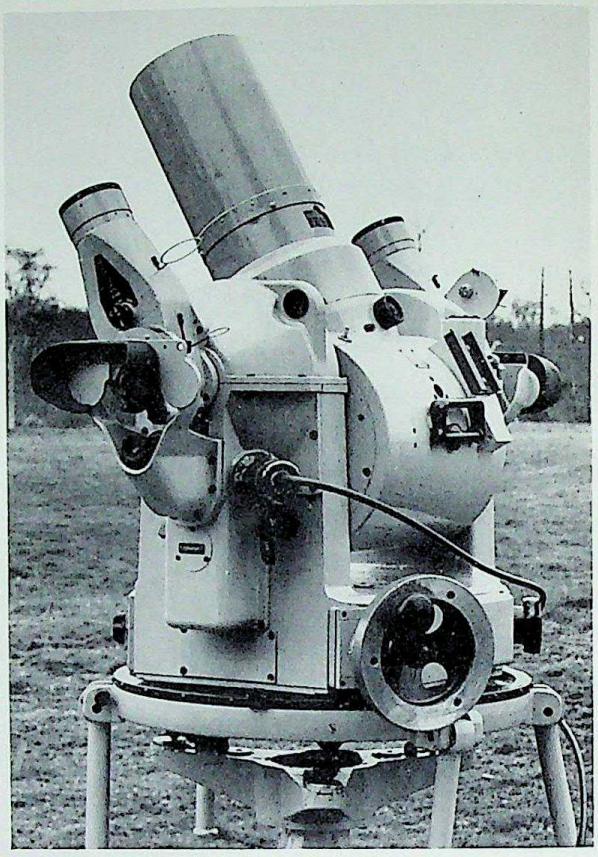

FIGURE 4 – *A kinetheodolite.*

Among the latter must be counted the kinetheodolite, which has given excellent results for the second and third Russian satellites. This optical instrument (figure 4) is controlled by two operators, one tracking in azimuth and the other in elevation. An internal flash-camera periodically photographs the scales, thus recording azimuth and elevation angles; at the same time the satellite is photographed through a long-focus lens mounted on the kinetheodolite. The purpose of the latter operation is to allow for errors in tracking: the displacement of the satellite's image on the photograph enables a correction to be made to the recorded azimuth and elevation angles. The exposures are controlled by accurate timing equipment at a rate of about five per second, so that during the course of a single transit some hundreds

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of readings may be recorded. These instruments were used extensively at various Ministry of Supply stations to establish an accurate orbit for Sputnik 2; under good conditions an accuracy of 20 seconds of arc was achieved, with a timing error of less than 20 milliseconds.

All the methods which have been described yield the satellite's displacement or direction as seen from a point on the Earth's surface—usually one of the observing stations—and this point is rotating with the Earth. But the Earth's diurnal motion plays no part in the satellite's progress: this is determined by the gravitational attraction directed (to a first order) towards the centre of the Earth, so that the simplest description of the orbit is to be found in a non-rotating reference frame with origin at the Earth's centre.

The transfer of information from topocentric to geocentric co-ordinates is somewhat involved and is best handled by a digital computer, which can also be programmed to minimize the effects of random observational errors. A programme evolved by R. H. Merson [7] for this purpose makes light work of the calculation: from kine-theodolite data, for example, the machine computes the best estimates of the orbital elements—the size and shape of the ellipse (for the orbit is approximately elliptical) and its orientation in space.

ORBITAL PERTURBATIONS

The satellites now circling the Earth carry a comprehensive range of instruments to sample the various physical conditions obtaining in space, with arrangements for transmitting the data back to Earth. Compared with the spate of information collected in this way the contribution from tracking data appears very meagre. This would indeed be so were the Earth strictly spherical and without an atmosphere: there would then be little interest in deriving accurate orbits for satellites near to the Earth. Apart from very small perturbations due to the Sun and other bodies in the solar system, the path would be an ellipse with one focus at the Earth's centre (figure 5); moreover, the ellipse would maintain a constant orientation in space. However, the presence of the atmosphere and the slight flattening of the Earth's surface at the poles both induce deviations from this simple state of affairs—deviations which, if measured accurately enough, can shed light on the phenomena which produce them. It is fortunate that the effects on the orbit of atmospheric drag and of the Earth's oblateness are quite distinct (at least to a first order), so that each effect can be unambiguously referred to its cause.

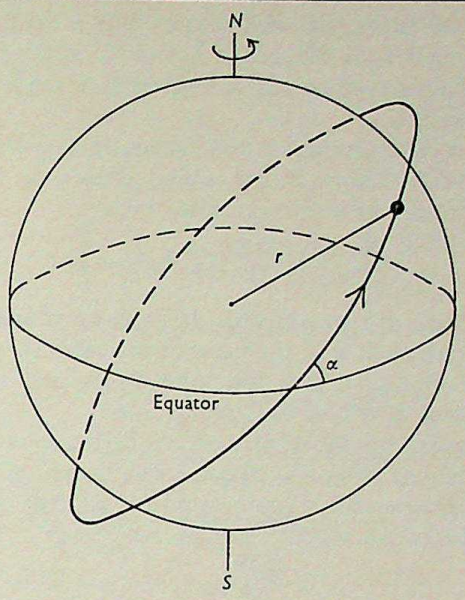

FIGURE 5 – *Inclination of the orbital plane.*

The non-uniform nature of the Earth's mass distribution results in a gravitational field which is not inversely proportional to the square of the radial distance. The external gravitational potential can be expressed as a series of spherical harmonics [8], the coefficients of each harmonic depending on the shape and mass distribution of the Earth; if then these coefficients can be determined, it is possible to deduce the figure of the Earth.

INFLUENCE OF THE EARTH'S OBLATENESS

The effects of such a non-central force on the orbit of a near Earth satellite has been studied theoretically [9], assuming the Earth to be an ellipsoid with a gravitational field which is independent of longitude and symmetrical about the equatorial plane. It is found that the orbital plane, instead of remaining in a fixed direction, rotates slowly about the Earth's axis. This precession, which is in a direction opposite to the satellite's motion, is of course quite distinct from the apparent precession observed from the Earth's surface, due to the rotation of the Earth. The expression for the rate of precession is rather lengthy, but to a good approximation it is

$$J\left(\sqrt{\frac{g}{R}}\right)\left(\frac{R}{\bar{r}}\right)^{3\cdot5}\cos\alpha$$

where g is the magnitude of the gravitational field at the equator, R the Earth's equatorial radius, $\bar{r}$ the harmonic mean distance of the satellite from the Earth's centre, and α the inclination of the

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orbit to the equatorial plane. J is a coefficient associated with the second harmonic in the gravitational expansion, and is a measure of the Earth's oblateness.

A secular variation also occurs in the orientation of the ellipse in the orbital plane; the major axis rotates at a rate given by

$$\frac{J}{2}\left(\sqrt{\frac{g}{R}}\right)\left(\frac{R}{\bar{r}}\right)^{3.5}(5\cos^2\alpha - 1)$$

The rate of rotation thus depends on the orbital inclination: it is in the same direction as the satellite's motion if α is less than 63·4°, and in the opposite direction for larger angles.

Apart from the secular effects, there are periodic departures from the ellipse. The largest of these disturbances has a period equal to half the orbital period of the satellite, and an amplitude

$$\frac{JR^2}{6\bar{r}}\sin^2\alpha$$

The first three Russian satellites all had an orbital inclination near 65°, so that the rotation of the major axis was very slow and difficult to measure, while the periodic changes in radial distance amounted to only one or two kilometres. The mean rate of rotation of the orbital plane, however, can be found accurately from observations over a long period, and offers the best hope of correlating theory and observation.

The theoretical and observed rates of precession for Sputnik 2 are compared in figure 6. The broken line is derived from theory, using the measured values of $\bar{r}$ and α and the accepted values for the geophysical constants. Both curves are subject to error—the first in the measured value of α and $\bar{r}$, and the second in the observed rate; but it turns out that the difference evident in the figure is significant. The observed rate suggests a lower value than is currently accepted for the ellipticity of the Earth.

However, the discrepancy may be due to other factors, such as atmospheric drag or asymmetry in the gravitational field. These effects have not yet been worked out in detail, but it is evident that, by accurately tracking satellites at different inclinations, it may be possible to improve our knowledge of the shape of the Earth, and as a consequence to define distances between points on its surface more precisely.

ATMOSPHERIC EFFECTS

The non-central gravitational field does not alter the general shape of the ellipse, apart from producing the small radial perturbation, but

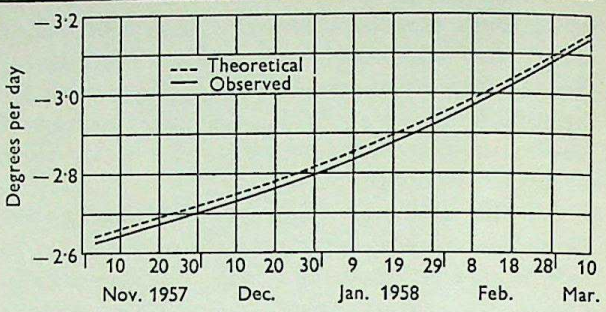

FIGURE 6 – *Rate of rotation of orbital plane of Sputnik 2.*

merely its orientation in space; the eccentricity and major axis are not affected, the perigee and apogee remaining at constant distances from the Earth's centre. The effect of the atmosphere is precisely the opposite: the major contribution of air drag is to distort the ellipse, with very little influence either on the rotation of the orbital plane or on the orientation of the ellipse in that plane.

For an eccentric orbit the retardation due to atmospheric drag varies during one revolution. The perigee heights of the first three Russian satellites were all in the region of 225 km, with apogees ranging from 900 km to 1900 km; and, since the air density falls away exponentially with height, the drag at perigee was enormously greater than at apogee for these satellites. The retardation at perigee causes a loss of height at the subsequent apogee; the orbit thus becomes less eccentric, but with very little change in perigee height. When the orbit has become circular, or almost so, the drag is more nearly constant, so that rather different conditions obtain: the satellite begins to descend in a spiral path, slowly at first, and then at an increasing rate until it is burnt up in the lower atmosphere.

The outward signs of these phenomena are a decrease in the orbital period and an increase in the satellite's speed—at first glance a puzzling feature, until it is remembered that the satellite

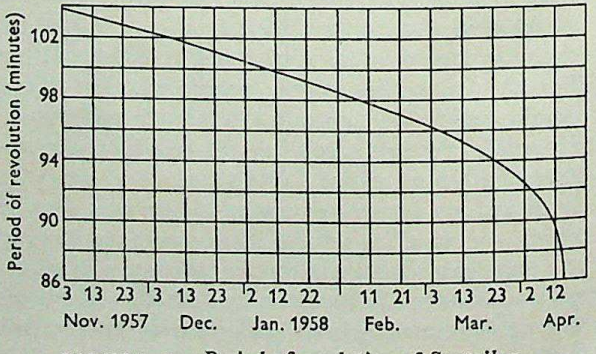

FIGURE 7 – *Period of revolution of Sputnik 2.*

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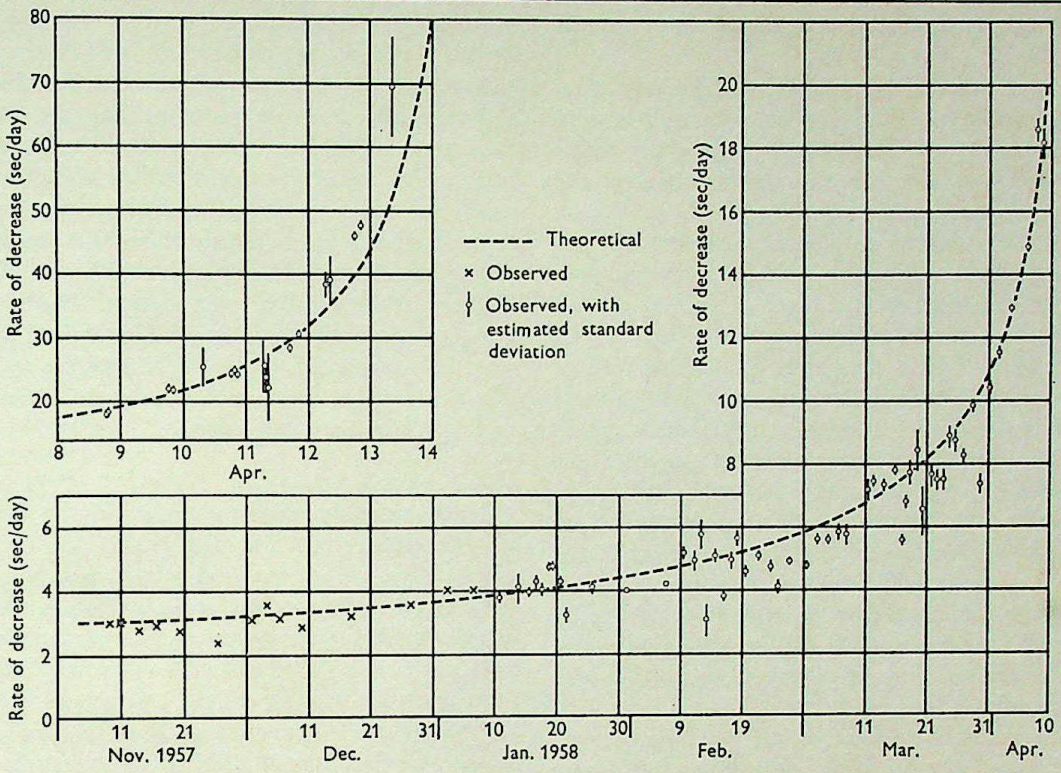

FIGURE 8 – *Rate of change of period of Sputnik 2.*

suffers a loss in height, and therefore in potential energy. This loss more than balances the increase in kinetic energy, so that there is a net loss of energy—that which is expended in overcoming air resistance.

The change in orbital period during the lifetime of Sputnik 2 is shown in figure 7; the period decreases at an increasing rate because of the greater air resistance at the lower altitude. It is evident that the rate of decrease is related to the air drag, which in turn depends on the density of the air and on the shape, size, and mass of the satellite.

The relationship is simply derived for a circular orbit. Since the gravitational intensity at a distance r from the centre of the Earth (considered uniform and spherical with radius R) is gR^2/r^2, the velocity v appropriate to a circular orbit at that distance is given by

$$\frac{v^2}{r}=\frac{gR^2}{r^2}$$

so that if the height decreases by a small amount Δr during one revolution, the gain in kinetic energy is

$$\tfrac{1}{2}mgR^2\left(\frac{1}{r+\Delta r}-\frac{1}{r}\right)\doteq\tfrac{1}{2}mv^2\frac{\Delta r}{r}$$

where m is the mass of the satellite. The loss of potential energy is

$$m\,.\,g\frac{R^2}{r^2}\,.\,\Delta r=mv^2\frac{\Delta r}{r}$$

which is precisely twice the gain in kinetic energy. The difference between these must equal the work done against the drag force F during one revolution:

$$\tfrac{1}{2}mv^2\frac{\Delta r}{r}=2\pi rF$$

The drag F may be written as

$$F=\tfrac{1}{2}\rho v^2SC_D$$

where ρ is the air density, S the area normal to the path, and C_D the drag coefficient. Thus

$$\frac{\Delta r}{r}=2\pi r\rho\frac{SC_D}{m}$$

The change in radius may be expressed in terms of the decrease in orbital period T by observing that

$$T=\frac{2\pi r}{v}=\frac{2\pi}{R\sqrt{g}}\,.\,r^{3/2}$$

from which

$$\frac{\Delta r}{r}=\tfrac{2}{3}\frac{\Delta T}{T}$$

The fractional change in orbital period can be measured very accurately, so that if SC_D/m is

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known it is possible to derive the density ρ at the height $r-R$.

The analysis can be extended to include non-circular orbits [10], giving in principle a continuous measure of air density; but for any shape other than a sphere it is difficult to estimate the effective area S, because of the changing attitude. At these very low densities there is virtually no aerodynamic damping, so pitching, yawing, and rolling motions, once acquired, tend to persist. The erratic behaviour of the orbital period is well illustrated in figure 8, which shows the rate of change of period for Sputnik 2. This satellite was roughly cylindrical, and the marked changes in brightness noted on most transits suggest that variations in orientation did in fact occur. The irregularity in period may of course partly reflect fluctuations in atmospheric density; but other disturbances—magnetic or electrostatic forces, for example, or meteoric bombardment—cannot be discounted, and systematic observations on many satellites are necessary if the phenomenon is to be fully explained.

The cumulative effect of air resistance eventually leads to the demise of a satellite. Its lifetime can be estimated from the change in orbital period [10], although the prediction is again somewhat uncertain, for the reasons just given.

IONOSPHERIC PHENOMENA

The propagation of electromagnetic waves in the ionosphere furnishes another area of investigation for which orbital data are highly desirable. Considerable attention has been devoted to ionospheric phenomena, but not until the launching of the first Earth satellite was it possible to study the transmission of waves from outside or within the ionized layers. A large store of data now exists and awaits analysis, but it is already evident that the paths by which signals are received may be extremely complicated. The value of such radio data is greatly enhanced if the location of the source with respect to the observing stations is accurately known, for then a comparison of the true direction with the apparent direction given by the radio signals indicates the amount by which the radio path is bent by the ionosphere. The bending will depend on the frequency used and on the angle of elevation, being least for overhead transits and greatest for low elevations. A series of radio observations of different frequencies and elevations, together with the true directions, should yield valuable information on the structure of the ionosphere.

Simultaneous radio and optical observations from the same station would be ideal for this purpose, but failing this it should be possible to establish the optical path sufficiently well from an accurately determined orbit.

CONCLUSION

The various tracking methods which have been touched upon are complementary rather than competitive. Each method has its advantages—the all-round view of the radio methods, the self-sufficiency of the radar system and its indifference to cloud cover, the immunity of optical observations from the vagaries of the ionosphere—and all are necessary to provide a clear picture of the satellite's behaviour and to further the study of the Earth and its environs.

Observations from one country, however, are most efficiently used in combination with similar data from other countries. In this connection the World Data Centres, set up by a special committee of the International Council of Scientific Unions to operate during the International Geophysical Year, should prove of great value; their functions are broadly to accept information from a multitude of observers, to analyse it, and to disseminate their results. Fortunately a satellite, whatever its country of origin, is freely available for tracking purposes, and, appropriately, the knowledge gained from a study of its orbit is of global interest.

ACKNOWLEDGMENTS

I am very grateful to my colleagues, Mr A. N. Beresford and Mr W. T. Blackband, who have provided much of the material concerning radio methods; and to Mr D. G. King-Hele and Mr R. H. Merson for their work on orbital perturbations in theory and practice.

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Book reviews

WHITE DWARF STARS

White Dwarfs, *by E. Schatzman. Pp. viii + 180. North-Holland Publishing Co., Amsterdam.* 1958. *Fl.* 19 *net.*

The interesting class of stars known as 'white dwarfs' consists of stars of very low intrinsic luminosity, fairly high surface temperature, very small size, and extremely high mean density. Because of their low luminosity, all the known white dwarfs (about 250 in number) are relatively near stars, but the distances of only 30 have been determined with precision.

The great interest of the white dwarfs lies in the fact that the material of which the star is composed does not obey the perfect gas laws because of its very high density. It is known as degenerate matter. This gives rise to many problems of great theoretical interest, which are fully dealt with in this monograph. The treatment is necessarily highly mathematical.

It appears that hydrogen cannot exist, and helium content must be very low, in the interior of a white dwarf. The internal core consists almost entirely of heavy elements, with a thin outer envelope, probably of almost pure hydrogen. The energy generated by thermonuclear reactions must be very small. Vibrational instability may result from the generation of energy by the hydrogen cycle in the envelope of the star and may give rise to recurrent explosions accompanied by the ejection of thin gaseous shells. The lifetime of a white dwarf is, however, very long, of the order of 10^{11} years.

The origin of white dwarfs is uncertain. Stars of mass much greater than that of the Sun could become white dwarfs only after losing much of their mass. It is possible that some white dwarfs have evolved from novae that have undergone a great number of recurrent outbursts. It is very improbable that the white dwarf stage is a normal product of stellar evolution. A number of binary and multiple stars contain a white dwarf in association with one or more normal stars, and white dwarfs are present in some relatively young clusters of stars.

The available information about the white dwarfs, both observational and theoretical, is summarized and discussed in this monograph, from which it is clear how incomplete is our present knowledge and understanding of their nature and evolution. It contains many minor errors and misprints which could have been avoided by more careful proof reading. H. SPENCER JONES

RADIO ASTRONOMY

The Exploration of Space by Radio *by R. Hanbury Brown and A. C. B. Lovell. Pp. xii+207. Chapman and Hall Ltd, London.* 1957. 35*s. net.*

Professor Lovell is the director of the Jodrell Bank Experimental Station and has been largely responsible for the successful completion of the giant 250 foot radio telescope which is just being brought into operation. In writing this book he and Hanbury Brown have presumably had in mind the provision of an up to date background knowledge of radio astronomy against which the future programme of the telescope can be seen in perspective. Such a volume is the more welcome because of the rapid progress of the science and the fact that the only other comprehensive text, 'Radio Astronomy,' by J. L. Pawsey and R. N. Bracewell, includes no work published since 1952.

The eleven chapters deal with the astronomical background; some properties of radio waves; techniques of radio astronomy; galactic and extragalactic radio emissions; the hydrogen line; the scintillation of radio stars; solar radio waves; meteors; radio and the aurora borealis; radio investigations of the moon, planets and the earth satellites; and the Jodrell Bank radio telescope. Not unnaturally those aspects of the subject which have been the particular concern of the Manchester teams, e.g. meteors, have had special emphasis.

This is not a book for the layman and a knowledge of some physics is essential for its understanding. More references would have been desirable and explanations about such techniques as 'post-detector correlation' should have been either fuller or not attempted. Occasionally concepts such as 'optical depth' are introduced without comment, but despite the necessary condensation it is, on the whole, a lucid account of radio astronomy.

The book is well printed and free from serious errors, apart from an unfortunate diagram in Figure 33. There are 27 plates, 10 of which illustrate the Jodrell Bank telescope.

J. R. SHAKESHAFT

MOLECULAR PHYSICS

Molecular Physics. *Vol. I, No. 1, pp. 98. Taylor and Francis Ltd, London.* 1958. 25*s. per part;* £4 15*s. subscription per volume.*

'Molecular Physics' is a new international journal intended to bring together papers on all aspects of the physics of molecules from all parts of the world. The truly international character is emphasized by the form of the Editorial Board which includes representatives of twelve countries; the languages used will be English, French, or German.

The first issue, dated January 1958, sets a very high standard. It includes contributions on nuclear magnetic resonance, dipole moments, rotational spectra, and critical opalescence in solid solutions. In addition there are a number of theoretical papers on electron distribution in molecular hydrogen, on the application of Prigogine's general theory of irreversible processes to quantum-mechanical systems, and on statistical mechanics.

The quality of the presentation and printing is excellent. R. E. RICHARDS

ATOMIC HYDROGEN SPECTRUM

The Spectrum of Atomic Hydrogen, *by G. W. Series. Pp. viii + 88. Oxford University Press, London.* 1957. 8*s.* 6*d. net.*

Throughout the history of quantum theory the hydrogen atom and its spectrum have played a major role. At each stage in the development of the theory applications were made to hydrogen, discrepancies between theory and experiment became apparent, and further theoretical development followed. This book gives a clear account of the successive theories and of the comparison of each theory with contemporary experimental knowledge. The book should be considered as a supplement to, rather than a substitute for, existing textbooks on quantum mechanics.

Of wider interest are the chapters on the experiments of Lamb and Rutherford and on the New Quantum Electrodynamics. These deserve to be read by the many scientists who have been unable to keep abreast of modern developments described in the periodical literature.

The title of the book is misleading in that students reading the book will get a rather one-sided view of the importance of the spectrum of atomic hydrogen in modern scientific research.

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Astrophysical aspects of the subject are not mentioned; the detection and interpretation of radio-frequency line radiation of cosmic atomic hydrogen is an outstanding achievement.

The publishers are to be commended on producing this excellent book at such a reasonable price. R. H. GARSTANG

NUCLEAR SCIENCE

Annual Review of Nuclear Science. *Vol. VII, edited by J. G. Beckerley. Pp. v + 496. Annual Reviews Inc., California.* 1957. $7 *net.*

This volume continues the editorial policy set out four years ago of informing specialists in one field about the situation in another, on the assumption that the specialist is willing 'to prepare himself with a minimum knowledge of the fundamentals of the new field.' This specialist, having enjoyed a splendid, though in places very theoretical, account of elementary particles, turned hopefully to Vertebrate Radiobiology (Lethal Actions and Associated Effects), only to find that physics and a social conscience could not get him beyond the first page. The minimum knowledge of fundamentals clearly extended well beyond a pass at Second M.B.

What then is the real purpose of this series as opposed to its avowed one? The paradoxical answer is that its greatest success lies in bringing the specialist up to date in his own field by slightly extending his horizon to include at least parts of the fields of his neighbours. Thus I found myself thoroughly at home with one article, profited greatly from two, exhibited an intelligent interest in three more—all six clearly excellent articles—but did not get beyond the first paragraph of the other six. This may be a respectable score, but are money and shelf space so plentiful that one can afford to buy so much that one can never hope to understand and that will be outdated in a very few years? L. R. B. ELTON

CRYSTAL PHYSICS

Experimental Crystal Physics *by W. A. Wooster. Pp. viii + 115. Clarendon Press, Oxford.* 1957. 18*s. net.*

Most books on the physical properties of crystals deal mainly with theory. This one, which is based on the course that has grown up in the Department of Mineralogy and Petrology in Cambridge, is concerned with laboratory work. The experiments are designed to illustrate principles rather than to achieve high accuracy, and are limited by the practical considerations that suitable crystals should be readily obtainable and the exercise should be capable of performance in an hour or two. A brief general discussion of each topic is followed by details of the apparatus and examples are given showing how the desired quantity is calculated. The largest section is devoted to optical constants of transparent and opaque substances. Dia-magnetic and para-magnetic properties, thermal conductivity and thermal expansion, piezo-electricity, pyro-electricity, and elasticity, are included by measurements of constants or qualitative demonstrations as appropriate. It is a very practical introduction to the subject.

H. M. POWELL

CATALYSIS

Contact Catalysis (*third edition*), *by R. H. Griffith and J. D. F. Marsh. Pp. x + 299. Oxford University Press, London.* 1957. 50*s. net.*

This, the third edition, re-written and enlarged, of Griffith's 'The Mechanism of Contact Catalysis,' presents in moderate compass a very large amount of factual information on adsorption and contact catalysis, and also outlines most of the modern theories, including the importance of lattice defects, the electronic factor emphasized by Dowden, and geometrical correspondence between adsorbed molecules and catalyst surface. There is also much practical information on the preparation of catalysts. The bibliography is excellent and the reader should have no difficulty in following up any theoretical or practical topic.

While the book is a mine of information, it is not easy to read consecutively, for it is largely a series of abstracts of experimental and theoretical papers. The argument is often condensed and does not flow smoothly from one section to the next, and the arrangement seems a bit hasty, with some overlapping between chapters. Correction of parts perhaps dictated is skimped in places: Tompkins appears as Tomkins and Wahba as Wobba! But some integration has been achieved and many suggestions for further work are made. The book is probably the best broad, as well as detailed, survey of this extremely complex field yet published, and should be invaluable to both academic and industrial chemists.

The authors' concluding remarks that 'the broad outlines of the subject are becoming clear,' even though 'the detailed mechanisms of individual reactions have rarely, if ever, been elucidated,' are probably justified.

N. K. ADAM

QUANTITATIVE INORGANIC ANALYSIS

Quantitative Inorganic Analysis, *by G. Charlot and Denise Bézier. Translated from the third French edition by R. C. Murray. Pp. x + 691. Methuen & Co. Ltd., London; John Wiley & Sons Inc., New York.* 1957. 84*s. net.*

Charlot and Bézier broke fresh ground when their book first appeared in 1944. In the revised third French edition (from which this translation has been made) they attempt a general survey of all the methods used in quantitative analysis. In the first part (322 pages) they deal with the various approaches, emphasizing different types of reactions and the fundamental theoretical principles. In the second part (316 pages) the determination of each element is considered in turn, a deliberate and critical choice of methods being attempted rather than a complete coverage. The last 46 pages include numerical data, bibliographies, and indexes.

The authors have, deliberately, not attempted to rival existing text-books which set out detailed procedures for quantitative determinations. It is a pity that, while they present a rich mine of information on other aspects of analysis, often in a novel and always in an interesting way, the treatment inevitably lacks detail and sometimes balance. Thus chromatography in all its aspects is dismissed in seven pages, emission spectrography and the uses of radioactivity each secures five pages, the determination of traces only three, and sampling only one page. The staccato style of the original must have been troublesome to translate and explains the occasional infelicity and inaccuracy.

Despite its weaknesses, this is a book to interest and stimulate all teachers and students and admirably suited to show the wide range and variety to be found in the continually expanding field of modern quantitative analysis.

H. IRVING

OXIDATION-REDUCTION TITRATIONS

Volumetric Analysis. *Vol. III*—Titration Methods: Oxidation-Reduction Reactions, *by I. M. Kolthoff and R. Belcher with the co-operation of V. A. Stenger and G. Matsuyama. Pp. ix + 714.*

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Interscience Publishers Inc., New York; Interscience Publishers Ltd, London. 1957. $15 *net.*

With the publication of volume III the treatise on Volumetric Analysis begun in 1942 by Professor Kolthoff is complete. The first volume dealt with theoretical fundamentals; volume II published in 1947 covered acid-base, precipitation, and complex-formation reactions. The present volume is devoted wholly to oxidation-reduction reactions.

The collective authorship to which Professor Kolthoff draws attention in his preface is a result of the withdrawal first of Dr Stenger, his collaborator in the two earlier volumes, and then of Dr Matsuyama. The work was finally completed with the help of Dr Belcher.

After a general introductory chapter there follow chapters on permanganate, ceric sulphate, potassium dichromate, iodometry applied both to inorganic and organic determinations, the Karl Fischer determination, potassium iodate, periodate, potassium bromate, and the hypohalites. The last two of the fifteen chapters deal with a number of strong reducing agents, (i.e. ferrous, titanous, stannous, mercurous, vanadous, and chromous salts), and with a miscellaneous collection of oxidizing and reducing titrants.

It is impossible in a short review to indicate in more detail the scope of this work but it will be appreciated that in a book of over 700 pages devoted solely to titrations based on oxidation-reduction reactions a most comprehensive coverage has been achieved. This volume will worthily command a place beside those earlier 'Kolthoff's on the analyst's shelves. The printing and binding are excellent.

R. C. CHIRNSIDE

QUANTITATIVE ORGANIC ANALYSIS

Quantitative Organic Analysis, *by J. S. Fritz and G. S. Hammond. Pp. ix + 303. John Wiley & Sons Inc., New York. Chapman and Hall Ltd, London.* 1957. 52*s. net.*

Comparatively few organic reactions proceed quantitatively and chemists are frequently faced with the problem of analysing mixtures of products. The present volume describes methods based upon selective functional-group reactions whereby individual components can often be estimated without the necessity of first isolating them. Most of these methods are quite new and the authors themselves have made many contributions to the subject.

Apart from chapter 2, which is rather heavy going, the text is written clearly and is enjoyable to read. A particularly valuable feature is the stress laid on the reasoning leading to a particular choice of method and to the way in which existing methods can be adapted or new methods devised to solve new problems. Indeed a whole chapter is devoted to the latter aspect. Other chapters deal with methods involving acid-base titrations in non-aqueous solvents, redox reactions, spectrophotometry measurement of physical properties, metal ion complexes, kinetics in analysis, separations and elemental analysis. Some detailed examples of procedures are given at the end.

Although primarily intended for teaching purposes in the United States, in Britain this book will find its greatest use among research workers and all professional analysts who wish to apply up-to-date methods to their problems.

JOHN F. W. McOMIE

ORGANIC SYNTHESES

Organic Syntheses, *vol. XXXVII, edited by J. Cason. Pp. vii + 109. John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1957. 32*s. net.*

This latest volume of the annual series which started as early as 1921 contains detailed instructions for the preparation of 31 organic compounds. These include benzfurazan oxide, benzoylacetanilide, 3-benzoylpyridine, 2-chloronicotinonitrile, diaminouracil hydrochloride, *trans*-2-dodecenoic acid, ethyl α-nitrobutyrate, *n*-heptamide, parabanic acid, and *ar*-tetrahydro-α-naphthol. The most novel syntheses are the preparation of norbornylene in up to 71 per cent yield from di*cyclo*pentadiene and ethylene, and of pseudopelletierine from 2-ethoxy-3:4-dihydro-2H-pyran by hydrolysis to glutaric dialdehyde and condensation with methylamine and acetone dicarboxylic acid.

The reviewer has long felt that the usefulness of these volumes would be enhanced by the addition to the accounts of certain of the preparations of a few words and references as to their synthetical potentialities. This is hardly necessary for compounds which are obviously general intermediates or starting points for a wide variety of synthesis, such as diethyl benzoylmalonate, ethyl benzoylacetate, glutaric acid and imide, oleoyl chloride, or stearolic acid, all of which are described in the present volume. On the other hand, it would clearly be of interest to have such notes on e.g. 2-chloro-2-methyl*cyclo*hexanone, 1-diethylamino-3-butanone, 3:4-dinitro-3-hexene, 1:4-diphenyl-5-amino-1:2:3-triazole, 4-ethyl-2-methyl-2-octenoic acid, ethyl *tert.*-butyl malonate, isophorone oxide, and trichloromethylphosphoryl dichloride, the preparations of which are also included in the volume under review.

W. BAKER

ORGANOMETALLIC COMPOUNDS

The Chemistry of Organometallic Compounds, *by E. G. Rochow, D. T. Hurd, and R. N. Lewis. Pp. vi + 344. John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1957. 68*s. net.*

There is a wide interest in organometallic compounds and this excellent book by well-known workers in this field of chemistry will be assured of a warm welcome. The authors state that they have tried to present the theoretical, factual, and practical aspects of organometallic compounds in a form which will be useful to the student and to the more general reader with a background of elementary chemistry. Obviously the authors have had to be selective in deciding upon subject-matter but in the opinion of the reviewer they have produced a volume that will be found of real value both to students and to research workers who may contemplate entering this field of investigation.

The book opens with an account of the general physical and chemical properties of organometallic compounds, and this is followed by a discussion of the general properties of the carbon-metal bond. The third chapter deals with the various methods which are available for the preparation of organometallic compounds. Against this background the authors systematically survey the compounds formed with the metallic and metalloidal elements. Of particular interest here are the organometallic compounds of the transition metals. (In 1951 the discovery of the *cyclo*pentadienyl compounds of iron excited great interest and opened up investigations which have resulted in an extensive organometallic chemistry of almost all the transition metals and most of the rare earths.) A very interesting chapter deals with applications of organometallic compounds in organic synthesis and in the section concerned with the simple olefines a discussion of the Ziegler process is

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included. The final chapter is devoted to an account of special types of organometallic compounds such as the fluorocarbon derivatives, olefine complexes, carbonyl compounds, cyanide and *iso*nitrile complexes and carbides. Although the hydrides fall outside the definition of organometallic compounds the authors mention them as they consider that a comparison of the metal hydrides with the corresponding organometallic compounds discloses some interesting features. With only a few exceptions organometallic compounds require special techniques for their preparation, purification, and handling. A detailed account of such experimental methods is not included in this volume but a short section entitled 'materials-handling techniques' contains some excellent advice. However, the novel method suggested for the disposal of highly volatile materials has obvious limitations. The text is liberally augmented with references to the chemical literature. The authors can be congratulated on achieving the aims they set themselves and producing a volume which looks well, reads well, and cannot fail to stimulate increased interest in this special field of chemistry.

W. WARDLAW

CLIMATOLOGY

Climatology (*second edition*), *by W. G. Kendrew. Pp. xv* + 400. *Clarendon Press, Oxford.* 1957. 42*s. net.*

The second edition of this well-known textbook incorporates many minor changes as well as some new sections on such subjects as jet streams and blocking anticyclones, all of which serve to bring the work up to date. The added index of place names will prove useful, but the index of subjects is still somewhat inadequate.

In the preface the author states that considerations of space exclude details 'such as the periods for which the mean values of climatological data used in the book are computed.' An omission such as that specified can hardly be justified by describing it as 'a point of subsidiary importance'; for unless the period covered by some mean value is stated, such a value has much less significance than is sometimes attributed to it. In particular, a comparison of mean values for different stations, which some of the graphs and tables invite, is certainly not justified unless such values cover the same period.

A few errors still remain, e.g. on page 71 the 'Mean monthly min.' temperature for July at Léopoldville and Kabete is in each case shown to be 1° lower than the 'Abs. extreme min.' However, this excellent introduction to modern climatology is much more than a mere collection of dull facts. It brings climatology to life, and it should certainly be possessed by every student of the subject.

F. G. HANNELL

CELL STRUCTURE

Macromolecules in Cell Structure, *by A. Frey-Wyssling. Pp. vii* + 112. *Harvard University Press, Cambridge, Mass.; Oxford University Press, London.* 1957. 40*s. net.*

The gap between the cell physiologist and biochemist on the one side and the morphologist on the other was wide and long unbridged. A substantial bridge has now been built with the aid of modern physical techniques such as X-ray diffraction and electron microscopy. In this building, none has been more active than Professor A. Frey-Wyssling. Frey-Wyssling was in this field before techniques were developed to their present stage and has used them with skill to test his own earlier hypotheses of structure at the submicroscopic level.

This book, based on the Prather Lectures delivered at Harvard University in 1956, traces briefly the development of the author's ideas, and supplements the longer account published in 1953 in his authoritative book, 'Submicroscopic Structure of the Cytoplasm.' Most of the examples are drawn from plants, largely from the work of the author or his colleagues in Zurich. Chapters on the fine structure of starch grains and of cell walls are followed by discussion of growth and differentiation in cell walls. The structure of chloroplasts and macromolecules in the cytoplasm are the last of two lectures in an interesting, well-illustrated, and readable book.

R. N. ROBERTSON

PROGRESS IN ENZYMOLOGY

Advances in Enzymology and Related Subjects of Biochemistry, *Vol. XIX, edited by F. F. Nord. Pp.* 457. *Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1957. $9.85 *net.*

This volume of Advances in Enzymology contains five authoritative reviews of exceptionally high standard. Vishniac, Horecker, and Ochoa describe various enzyme systems involved in metabolic pathways in photosynthetic cells under the heading 'Enzymic Aspects of Photosynthesis.' H. S. Mason's article on the mechanisms of oxygen metabolism is a comprehensive monograph of over 150 pages citing 842 references. The chemical mechanisms of three types of enzyme-catalysed oxidation reaction are described, oxygen transfer, electron transfer, and mixed function oxidases. The author is clearly more expert on oxygen transfer and mixed function oxidases than on electron transfer and the latter is treated only briefly. Wieland and Pfleiderer in an article *Aktivierung von Aminosäuren* give a very good account of the mechanisms of reactions of the type which are likely to be involved in the formation of peptide bonds during the biosynthesis of proteins. Kimmel and Smith in a review of the properties of papain describe a subject which is almost entirely their own. It is a nice object lesson of how the study of the physical and chemical properties as well as of the reaction mechanism of an isolated enzyme should be developed. The final article by Braunstein on *Les voies principales de l'assimilation et dissimilation de l'azote chez les animaux* presents a very scholarly review of up to date ideas on nitrogen metabolism. This is altogether a most valuable volume.

H. GUTFREUND

MARINE ALGAE

Marine Algae of the Northeastern Coast of North America (*second revised edition*), *by W. R. Taylor. Pp. viii* + 509. *The University of Michigan Press, Ann Arbor.* 1957. $12.50 *net.*

The first edition of this work appeared in 1937 and was the only complete account of the marine algae found along the North American Atlantic seaboard from Virginia to the Arctic Circle. In the intervening years it has come to be regarded as a classic in the phycological literature. This revised edition contains descriptions of more than 500 different marine plants found in the littoral and sublittoral zones, including a number of recently discovered species. The text has also been broadened to cover the results of current research on taxonomy and nomenclature.

Professor Taylor gives a history of the earlier work in this region, descriptions of the changes in the algal flora from one latitude to another, and directions for collection and preservation. Keys for orders, families, genera, and species are included in the descriptive catalogue. An excellent bibliography, covering 18 pages, is also included.

F. N. WOODWARD

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Short notices of books

(These notices are descriptive rather than critical and are designed to give a general indication of the nature and scope of the books.)

Daedalus, *edited by P. Rieff. Pp.* 140, *American Academy of Arts and Sciences. Quarterly, subscription* $6.50 *per annum.*

The winter, 1958, issue of this journal is Volume 87, No. 1 of the Proceedings of the American Academy of Arts and Sciences, but is, in fact, the first issue in a new format. This issue is a special one devoted to 'Science and the Modern World View', and it, and subsequent issues are intended to link various sections of the academic world, and are directed at the educated public.

Atlas of the Sky, *by V. de Callataÿ. Pp.* 157. *Macmillan & Co. Ltd, London; St Martin's Press, New York.* 1958. 65*s. net.*

Sir Harold Spencer Jones has translated this book from the French, and written a preface. The book is intended to enable the amateur astronomer, working without a telescope, to recognize the constellations and stars he can see. The illustrations include a number of plates which are representations of the sky in which the stars are depicted as white spots, graded according to their apparent brightness, on a black background, as well as many line drawings. There is also a text giving an elementary outline of modern astronomy.

Nuclear Radiation Detection, *by W. J. Price. Pp. vii*+382. *McGraw-Hill Book Co. Inc., New York and London.* 1958. 70*s. net.*

The intention of this book is to collect the basic information on all the important nuclear radiation detectors in use today; specific information on the application of the various detectors is given, to enable the reader to select and, often, to apply the equipment. While practically all the equipment described is available commercially, emphasis is given to the principles involved, with a view to making the book useful to the reader who desires to design his own equipment.

The Scaling-up of Chemical Plant and Processes, *edited by B. J. M. Pirie. Pp.* 136. *The Institution of Chemical Engineers, London.* 1958. £3 *net post free* (£2 *net post free to members of sponsoring bodies*).

This is the proceedings of a joint symposium that was held in London in 1957 and sponsored by the chemical engineering group of the Koninklijk Instituut van Ingenieurs, the chemical technology section of the Koninklijke Nederlandse Chemische Vereniging, and the Institution of Chemical Engineers. The subject is considered both generally and in detail, with a total of thirteen papers dealing with, amongst other things, the use of kinetics, the economic aspects, and with the scaling-up of particular pieces of apparatus, such as stirred slurry reactors, solvent extraction plant, gas absorption apparatus, and continuous filtration equipment.

Phenolic Resin Chemistry, *by N. J. L. Megson. Pp. vii*+323. *Butterworth's Scientific Publications, London; Academic Press, Inc., New York.* 1958. 65*s. net.*

The field covered in this book is that of the pure chemistry, as opposed to the technology, of the phenolic resins, mainly limited to those involving formaldehyde, and referring in detail only to developments since 1939. There are chapters on the phenolic alcohols, the ammoniacal and related phenol formaldehyde condensations, rational syntheses, and rates of reaction, amongst others. The author's aim has been to make a detailed survey and the range of references is very wide, including those in not widely understood languages, such as Japanese.

Population Studies: Animal Ecology and Demography. *Pp. xiv*+437. *Biological Laboratory, Cold Spring Harbor, New York.* 1957. $8 *net.*

This is volume XXII of the series of Proceedings of the Cold Spring Harbor Symposia on Quantitative Biology. Apart from introductory and closing speeches, there are 33 papers published here on the historical study of human populations, demographic theory, the experimental and theoretical study of animal populations, and other subjects connected with communities and populations. The papers include bibliographies.

Leptospirosis in Man and Animals, *by J. M. Alston and J. C. Broom. Pp. xii*+367. *E. & S. Livingstone Ltd, Edinburgh and London.* 1958. 40*s. net.*

The authors of this book aim at giving a survey of what is at present known about the leptospires and the diseases they cause in man and animals, and of the probable lines of future research; attention is given to the increasing frequency with which the disease occurs in some places, and the new serotypes involved. A chapter on the legal aspects of leptospirosis, by C. J. A. Doughty, has been added to make the survey comprehensive.

Scientific Glassblowing, *by E. L. Wheeler. Pp. xxii*+478. *Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1958. $9.75 *net.*

The modern glassblower can well be an understanding co-worker with the research chemist, and this book is designed to instruct the professional glassblower so that he may attain this level. It is written by a professional, and describes the cross-fire technique as used in America, giving instructions that start from simple joins and end with complicated vacuum and distillation apparatus. The book also has chapters on modern techniques, such as chromatography and distillation, that the author considers to be necessary background knowledge for the professional glassblower.

Immunology, *Vol.* 1, *No.* 1, *edited by J. R. Marrack. Pp. iii*+87. *Published quarterly by Blackwell Scientific Publications, Oxford.* 1958. 18*s. net per copy. Subscription* 60*s. per annum.*

This is the official journal of the British Society for Immunology, and this number contains eight papers on subjects related to immunity—three on skin homografts and the remainder on antibodies, their properties and uses. The editorial points out that immunology covers a wider area than this—that its boundaries, in fact, are indefinite and anticipates papers both on the wider aspects, and on the limited one of freedom from injury, where much interesting work is being done at present.

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Notes on contributors

V. V. BELOUSSOV,

Was born in 1907. He is a Correspondent Member of the Academy of Sciences of the U.S.S.R. and is Chairman of the Soviet Academy Committee on Geodesy and Geophysics. He has worked mainly in the field of tectonics, studying in particular the history of the wave movements of the earth's core and the geological development of the Caucasus range. The main results of his work are embodied in Общая Тектоника (General Tectonics), Moscow 1948. He is also the author of Очерки Геохимии природных Газов (Essays on the Geochemistry of Natural Gases), Leningrad, 1937, and Большой Кавказ (The Great Caucasus), Leningrad, 1938–40, besides other works.

ANTOINETTE PIRIE,
M.A., Ph.D.,

Was born in London in 1905 and educated at Newnham College, Cambridge. She worked in the Biochemical Laboratory, University of Cambridge, under Professor Sir F. G. Hopkins until 1940, when she moved to London and then Oxford to work on the biochemistry of the eye. She is now Reader in Ophthalmology, University of Oxford. Her research interests include the biochemical changes that initiate cataract and the relation of the biochemistry of the eye to the functions of its separate parts. Together with Dr Ruth van Heyningen she has written 'The Biochemistry of the Eye'.

M. F. PERUTZ,
Ph.D., F.R.S.,

Was educated at the Universities of Vienna and Cambridge and is Director of the Medical Research Council's Molecular Biology Unit at the Cavendish Laboratory, Cambridge. He is also a Reader at the Davy Faraday Research Laboratory at the Royal Institution in London. His main field is X-ray crystallography, particularly research into the structure of haemoglobin and the elucidation of the physical and chemical properties of protein crystals. A love of mountaineering and ski-ing brought him into contact with glacier research, to which he has made several contributions.

WŁODZIMIERZ HUBICKI,
M.A., Ph.D.,

Was born in Borysław in 1914 and educated at the University of Cracow. In 1938 he became a research assistant in inorganic chemistry at this University and during the war was a teacher in the Technical School at Cracow. Since 1947 he has been professor of inorganic chemistry at the Marie Curie-Skłodowska University at Lublin. From 1949 until 1952 he was the Dean and since 1956 the Vice-Rector. He is also Professor at the Institute for the History of Science of the Polish Academy of Science and the head of the section for history of chemistry. He is the author of many research papers on inorganic and analytical chemistry and on the early history of chemistry.

H. P. BROIDA,
A.B., A.M., Ph.D.,

Was born in Aurora, Colorado, in 1920 and studied at the Universities of Colorado and Harvard. In 1949, after teaching at Wesleyan and at Harvard, he joined the National Bureau of Standards where he has done research on the spectroscopy of flames, chemical kinetics, diatomic spectra, isotope analysis, medical instrumentation, and more recently, as an outgrowth of his work in flames, on free radical production and stabilization. In 1952–53 he spent a year at Imperial College, London, as a Guggenheim Fellow and in 1956 he received the Arthur S. Flemming award of the Washington Junior Chamber of Commerce.

E. G. C. BURT,
B.Sc., A.M.I.E.E.,

Was born in Somerset in 1922 and educated at Yeovil School and London University (Queen Mary College). He graduated in electrical engineering in 1943; served as an R.A.F. officer from 1943 to 1947, some of this time being spent at the Royal Aircraft Establishment in the Radio Department and Instruments Department. Joined the Guided Weapons Department in 1947 as Scientific Officer. In 1955 he became Senior Principal Scientific Officer and Head of the Dynamic Analysis Division, which is concerned with the theoretical aspects of the guidance and control of missiles and satellites. He has published papers on analogue computing techniques, on the theory of servo mechanisms, and on the effects of random disturbances in such systems.

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